PROCEEDINGS

OF THE

American Philosophical Society

HELD AT PHILADELPHIA

FOR

PROMOTING USEFUL KNOWLEDGE

VOLUME XLV

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AMERICAN PHILOSOPHICAL SOCIETY

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VOL. XLV JANUARY–APRIL, 1906. No. 182.

Stated Meeting January 5, 1906.

President SMITH in the Chair.

An invitation was received from the Royal Geographical Society of Australasia, announcing that it would celebrate in June, 1906, the twenty-first anniversary of its foundation and inviting this Society to participate. On motion the President was authorized to appoint a delegate to represent the Society thereat.

The Judges of the Annual Election of Officers and Councillors reported that an election had been held on the afternoon of this day and that the following named members had been elected to be officers for the ensuing year:

President

Edgar F. Smith.

Vice-Presidents

George F. Barker, William B. Scott, Simon Newcomb.

Secretaries

I. Minis Hays, Arthur W. Goodspeed,
Edwin G. Conklin, Morris Jastrow, Jr.

Curators

Charles L. Doolittle, William P. Wilson, Albert H. Smyth.

Treasurer

Henry La Barre Jayne.

Councillors to Serve for Three Years

Patterson Du Bois, Ernest W. Brown,
Samuel Dickson, William Keith Brooks.

The Annual Report of the Treasurer was presented.

Stated Meeting January 19, 1906.

President SMITH in the Chair.

Mr. Burnet Landreth read a paper entitled "A Case of Persistent Vitality in Seeds" (see page 5), which was discussed by Dr. W. P. Wilson, Professor Conklin and Dr. Ravenel.

Dr. John Marshall made some "Observations on the Venom of the Rattlesnake," which was discussed by Dr. W. P. Wilson, Professor Willis, Mr. J. E. Whitfield, Dr. Ravenel and Dr. Fetterolf.

A CASE OF PERSISTENT VITALITY IN SEEDS.

By BURNET LANDRETH.

(*Read January 19, 1906.*)

Mr. Watson, Curator Royal Botanic Gardens of Kew, published in the *Gardeners' Chronicle* of the eleventh of February, 1905, his opinion that seeds hermetically sealed were injured in vitality. No doubt he is correct to a degree, but commercially he is wrong, as those merchants practically engaged in shipping seeds through, or to, damp climates, as via the Suez Canal to India, have had just the opposite experience as compared with seeds not hermetically sealed.

The advantage of air-tight containers for the transportation through, or the keeping of seeds, in tropical countries, has also been proven nearer home, as, not only in Central America and Mexico, but in our own states bordering on the Gulf of Mexico, where it is well known that seeds not hermetically sealed will lose 50 per cent. to 60 per cent., and even 70 per cent., of vitality in a single summer; in the language of the southern seedsman, "they sweat to death."

But just here is a novel record as respects exposed seeds in a dry and arctic climate, an incident without any paralleling features as to a prolongation of vitality.

The seeds referred to, if kept at Bloomsdale Farm, where they were grown, through the period of sixteen years, would not have possessed any vitality, while in this case, under a continuously low temperature, one of the two varieties saved, the radish, grew up to 50 per cent. upon being returned to the United States; this, I infer, from the complete arresting of transpiration in the dry and cold atmosphere of the very far north.

This is a record of scientific interest, a contribution to the store of vegetable physiology, a demonstration never before attainable, and not likely ever again to be repeated. It is, and will remain, unique.

The following is the statement of Dr. Dedrick of the Peary Expedition of 1901:

"WASHINGTON, NEW JERSEY,
"15th November, 1905.

"MR. BURNET LANDRETH,

"*Dear Sir:*—The incidents of the finding of certain seeds abandoned by Lieutenant Greely at Fort Conger, 490 miles from the pole, were as follows:

"In January, 1899, the expedition of Lieutenant Peary, of which I was surgeon, discovered Fort Conger, 81° 44′, or about 490 miles from the pole. This station was abandoned in 1883, sixteen years, and among the articles reclaimed by the Peary party were a lot of seeds in packages bearing your name. These seeds were sealed up in the usual flat paper packets as issued by your establishment (I send you some of the identical seed packets) and were found in an open box in the loft or attic of Fort Conger, where they had rested sixteen years, well sheltered from rain and snow, but exposed to a winter temperature of 60° to 70° below zero.

"In April, 1899, with the seeds in my possession, we journeyed by sledge over the ice some 300 miles south to our ship, from whence I sent the seeds home that season, where they remained unplanted until the spring of 1905.

"The seeds from two of these packets, one of lettuce and one of radish, I planted in my garden at Washington, New Jersey. The lettuce seed failed entirely to germinate, but about one half of the radish seeds germinated and reached perfection in size, and even reproduced seed. The photograph which I enclose was taken from one of the roots, grown during the summer of 1905, after it had reached full development, and the seeds sent you are the produce of the same roots.

"This retention of germinative force, under the conditions of sixteen winters of exceedingly low temperature (we found it 70° below zero at Fort Conger) and during the five years subsequently when brought back to the United States, twenty-one years in the total, is to me most extraordinary, and this record is another added to the many valuable contributions to science made by Lieutenant Peary's four years' expedition.

"THOMAS S. DEDRICK, M.D."

Explanatory of the taking of the seeds to the far north, is the

following letter from General A. W. Greely, now Chief Signal Officer of the Army:

"WAR DEPARTMENT,
"WASHINGTON, D. C.,
"October 23, 1905.

"MR. BURNET LANDRETH, Bristol, Pennsylvania.

"*Dear Sir:*—Referring to your letter of October 21, I have to state that I took north with me in 1881, in connection with the Lady Franklin Bay Expedition, various seeds. It is my impression, although I am not certain on this point, that they came from your own seed farm. Attempts were made at Fort Conger, 81° 44′ north, 64° 45′ west, to raise crops of lettuce, cabbage, radish, etc., with the desire of adding fresh vegetables to the dietary of the expedition, for their antiscorbutic qualities.

"Despite considerable care these efforts were not successful, the general opinion being that the soil was too strongly saturated with alkalies to suit these crops.

"Judging from the experience of my own expedition, the seeds which were brought back by Surgeon Thomas Dedrick must have been subjected nearly every winter since 1883 to temperatures of 60° below zero, Fahrenheit, and probably during the summer to temperatures approximating 60° above zero, Fahrenheit.

"Yours truly,
"A. W. GREELY."

No seeds, so far as on record, ever had such a prolonged or severe test as to their vitality as these which Lieutenant Greely took to the very far north, seeds which laid at Camp Conger, with other abandoned property, for sixteen years, or until 1899, when the north polar expedition under Lieutenant Peary found the camp.

Experiments in the laboratory have been made by exposing seeds to the influences of liquid air in temperatures 40° and 50° below zero, but never has the opportunity existed, and possibly there never will, to reclaim seeds after seventeen years' exposure to an arctic temperature every winter of 60° below zero.

The following are some extracts upon the subject of the retention of germinative force of radish seeds:

1. From the Seed Division of the United States Department of Agriculture: "We have not conducted any experiments along these lines."

2. From the Seed Division of Cornell University: "Our director of twenty years ago, Dr. Sturtevant, made some experiments to determine the longevity of radish seeds, and reported that starting with one-year-old seed at 71 per cent., it was at two years 57 per cent., at three years 49 per cent., at five years 37 per cent., six years 12 per cent., seven years 3 per cent., twelve years 0 per cent."

3. From the Department of Agriculture, Ottawa, Ontario: "We have no experiments to report. Our observations are that a dry cold atmosphere has little or no influence upon northern-grown seed. The effect of cold depends upon the amount of moisture surrounding the embryo. Northern-grown glutinous wheats are very resistant to cold."

The summary of these communications, and many others, being to the effect that never before has been presented any similar opportunity under which can be observed the effect of intensely low and prolonged cold as preserving and greatly extending the germinative force of seeds. These seeds certainly were harvested the summer of 1880 or earlier, consequently were twenty-three years old when the crop was grown. Just here the thought occurs to me, can it be possible that the electrically charged atmosphere, so constant in far northern regions, has the effect of prolonging germinative force. All arctic explorers observe that the atmospheric electric currents add quite one hundred per cent. to the rapidity of plant growth, and to the development of a miraculous brilliancy of color and strength of perfume.

Bristol, Penn.

Stated Meeting February 2, 1906.

President SMITH in the Chair.

Dr. Charles H. Frazier, a recently elected member, was presented to the Chair and took his seat in the Society.

An invitation was presented from the Academy of Science of St. Louis, inviting the Society to participate in the Commemoration of the Fiftieth Anniversary of its foundation, on March 10 next, and the President appointed Dr. William Trelease to represent the Society on this occasion.

The decease of the following members was announced:

Professor A. Thury, of Geneva, Switzerland, on January 19, 1905, aged 82 years.

Professor Peter R. v. Tunner, of Leoben, Austria, on June 8, 1897, aged 89 years.

The following papers were read:

"The Uses of Plants by the American Indian," by Mr. Frederick V. Coville, which was discussed by Mr. Jayne.

"Some Results from the Drift-Cask Experiment in the Arctic Ocean," by Mr. Henry G. Bryant.

Stated Meeting February 16, 1906.

President SMITH in the Chair.

The decease was announced of Professor Fredolin Sandberger, of Wurzburg, Bavaria, on April 11, 1898, aged 71 years.

Professor William A. Lamberton read a paper entitled "Some Points in the Work of St. Paul," which was discussed by Mr. Goodwin and Mr. Wood.

Stated Meeting March 2, 1906.

President SMITH in the Chair.

The decease was announced of Prof. Samuel P. Langley, on February 27, 1906, æt. 71, at Aiken, S. C.

Dr. David L. Edsall read a paper on "Some Recent Studies of Digestion," which was discussed by Prof. E. G. Conklin.

Stated Meeting March 16, 1906.

President SMITH in the Chair.

The decease was announced of Mr. Clarence H. Clark, at Philadelphia, on March 13, 1906, æt. 73.

Dr. Edward A. Spitzka read a paper entitled "The Anatomy of the Human Brain, with Special Reference to the Mental Functions —as illustrated by the Brains of Men Notable in the Professions, Arts and Sciences." Discussed by Dr. Dercum and President MacAlister.

Stated Meeting April 6, 1906.

President SMITH in the Chair.

The decease of the following members was announced:

Mr. William W. Jefferies, at New York, on February 23, 1906, æt. 86.

Mr. John Vaughan Merrick, at Philadelphia, on March 28, 1906, æt. 78.

Dr. Henry M. Chance read an obituary notice of Prof. J. Peter Lesley.

Mr. B. H. E. Groth presented a paper on "The History, Structure and Growth of the Sweet Potato," which was discussed by Mr. Goodwin and Mr. Landreth.

Dr. J. M. Macfarlane exhibited some species and hybrids of *Sarracenia* in flower from the Botanical Garden of the University of Pennsylvania.

General Meeting April 17, 18, 19 and 20, 1906.

April 17—Evening Session.

President SMITH in the Chair.

The opening session was devoted to the celebration of the two hundredth anniversary of the birth of Benjamin Franklin, and the proceedings will be found in the *Franklin Bi-centenary Volume.*

April 18—Morning Session.

The following papers were read:

"The Statistical Method in Chemical Geology," by Frank Wigglesworth Clark, Sc.D., of Washington, which was discussed by Mr. Joseph Wilcox.

"On a Possible Reversal of the Deep Sea Circulation and its Effect on Geological Climates," by Prof. Thomas C. Chamberlin, of Chicago, which was discussed by Professors Newcomb, Morris Davis, Chamberlin and others.

"Elementary Species in Agriculture," by Prof. Hugo deVries, of Amsterdam, Holland, which was discussed by Professors Conklin, Osborn and Dr. McDugal.

"An International Southern Observatory," by Prof. Edward C. Pickering, of Cambridge, Mass., which was discussed by Professor Michelson and Dr. Brashear.

"The Figure and Stability of a Liquid Satellite," by Sir George Darwin, K.C.B., F.R.S., of Cambridge, England.

Executive Session—12.30 o'clock.

The pending nominations for membership were read and spoken to, and the Society proceeded to an election.

Afternoon Session.

The tellers of election reported that the following candidates had been elected to membership:

Residents of the United States.

Hon. Joseph Hodges Choate, LL.D., D.C.L. (Oxon.), New York.

Henry Herbert Donaldson, Ph.D., Philadelphia.

Russell Duane, Philadelphia.

David Linn Edsall, M.D., Philadelphia.

John W. Harshberger, Ph.D., Philadelphia.

Charles S. Hastings, Ph.D., New Haven, Conn.

William Francis Hillebrand, Ph.D. (Heidelberg), Washington.

Charles Rockwell Lanman, LL.D., Cambridge, Mass.

Franklin Paine Mall, M.D., LL.D., Baltimore.

Ernest Fox Nichols, D.Sc., New York City.

Hon. Elihu Root, LL.D., Washington.

Thomas Day Seymour, LL.D., New Haven, Conn.

Edward Bradford Titchener, M.A., Oxford; Ph.D., Leipsic, Ithaca, New York.

Otto Hilgard Tittmann, Washington.

Arthur Gordon Webster, Ph.D. (Berlin), Worcester, Mass.

Foreign Residents.

Adolf Engler, Ph.D., Berlin.

Dr. Hendrik Antoon Lorentz, Leyden, Holland.

Dmitri Ivanovitch Mendeleff, St. Petersburg.

Theodor Nöldeke, Ph.D., Strassburg.

August Weismann, Freiburg.

The following papers were presented:

"Form Analysis," by Prof. Albert A. Michelson, of Chicago.

"The Present Position of the Problem concerning the First Principles of Scientific Theory," by Prof. Josiah Royce, of Cambridge, Mass.

"The Human Harvest," by President David Starr Jordan, of Stanford University, Cal.

"On Positive and Negative Electrons," by Prof. H. A. Lorentz, of Amsterdam.

Evening Session.

Vice-President BARKER in the Chair.

(See Franklin Bi-centenary Volume.)

April 19.

(See Franklin Bi-centenary Volume.)

April 20—Morning Session.

(See Franklin Bi-centenary Volume.)

Afternoon Session.

The following papers were presented:

"The Elimination of Velocity-Head in the Measurements of Pressures in a Fluid Stream," by Prof. Francis E. Nipher, of St. Louis.

"Old Weather Records and Franklin as a Meteorologist," by Prof. Cleveland Abbe, of Washington.

"Was Lewis Evans or Benjamin Franklin the first to recognize that our North-east Storms come from the South-west?" by Prof. William Morris Davis, of Cambridge, Mass.

"Notes on the Production of Optical-Planes of large Dimensions," by Dr. John A. Brashear, of Allegheny, Pa.

"Repetition and Variation in Poetic Structure," by Prof. Francis Barton Gummere, of Haverford, Pa.

"The Herodotean Prototype of Esther and Sheherazade," by Prof. Paul Haupt, of Baltimore, Md.

"Heredity and Variation, Logical and Biological," by Prof. Wm. Keith Brooks, of Baltimore.

"Notes on a Collection of Fossil Mammals from Natal," by Prof. William B. Scott, of Princeton.

"The Use of Dilute Solutions of Sulphuric Acid as a Fungicide," by Prof. Henry Kraemer, of Philadelphia.

Prof. Arthur G. Webster and Prof. Charles R. Lanman, newly elected members, were presented to the Chair and took their seats in the Society.

Letters of acceptance of membership were read from Dr. Henry A. Donaldson, Mr. Russell Duane, Dr. David L. Edsall and Prof. Franklin P. Mall.

It was ordered that a message of condolence be sent to Madame Curie, on the death of her husband.

The following resolution, offered by Dr. Hays, was unanimously adopted:

As the Society has heard with profound grief of the appalling disaster that has befallen San Francisco and its neighboring towns, it desires to express its deep sympathy for their citizens, and particularly for its sister society, the California Academy of Sciences, and as a slight evidence of its sympathy, the Secretaries be instructed to send to the California Academy of Sciences a complete set of the Transactions and the Proceedings of the American Philosophical Society, so far as the same may be available, so soon as the California Academy of Sciences is prepared to re-form its library.

THE STATISTICAL METHOD IN CHEMICAL GEOLOGY.

By F. W. CLARKE.

(*Read April 18, 1906.*)

In an essay upon the relative abundance of the chemical elements, published some sixteen years ago,[1] I attempted to apply the statistical method to ascertaining the average composition of the earth's crust. Since that time the data have been repeatedly revised, both by myself and by others, and the results obtained have found applications which I did not anticipate. The composition of the lithosphere has furnished, so to speak, a sort of base-line to which other computations could be referred; the figures, therefore, have acquired a peculiar importance, and it has become desirable to determine, more critically than heretofore, the degree of their validity. To discuss the nature of the averages and to consider how far they may be utilized is the purpose of the present communication. In order to accomplish this purpose I must restate, very briefly, the main points of the original argument.

As a first step in the discussion, it was necessary to assume a definite mass of matter as available for statistical analysis. That mass included the atmosphere, the ocean and a certain portion of the lithosphere; the portion, namely, that may be supposed to lie within our reach. For the last item it was assumed that a shell having a thickness of ten miles below sea-level would represent known material; in other words, that it would consist of rocks essentially identical in general character with those which we can study at the surface. How much thicker the rocky crust of the earth may be is another question, upon which we do not need to enter. It is only the *known* material which concerns us now; and some of that is brought to us by volcanoes from depths far below any to which we can penetrate directly. The eruptive rocks enable us to determine what sort of matter lies below the immediately observable surface.

[1] *Bull. Philos. Soc. Washington*, vol. 11, p. 131, Oct. 26, 1889. Also in U. S. Geol. Survey Bull. 78, p. 34.

I am indebted to Dr. R. S. Woodward for data relative to the volume of matter which is thus taken into account. The volume of the ten-mile rocky crust, including the mean elevation of the continents above the sea, is 1,633,000,000 cubic miles; and to this material we may assign a mean density not lower than 2.5, nor much higher than 2.7. The volume of the ocean is put at 302,000,000[1] cubic miles, and I have given it a density of 1.03, which is a trifle too high. The mass of the atmosphere, so far as it can be determined, is equivalent to that of 1,268,000 cubic miles of water, the unit of density. Combining these data, we get the following expression for the composition of the known matter of our globe.

	Density of Crust. 2.5.	Density of Crust. 2.7.
Percentage of atmosphere.....	.03	.03
Percentage of ocean.....	7.08	6.58
Percentage of solid crust.....	92.89	93.39
	100.00	100.00

In short, we can regard the surface layer of the earth, to a depth of ten miles, as consisting very nearly of 93 per cent. solid and 7 per cent. liquid matter, treating the atmosphere as a small correction to be applied when needed.[2] The figure thus assigned to the ocean is probably a little too high, but its adoption makes an allowance for the fresh waters of the globe, which are too small in amount to be estimable directly. Their insignificance may be inferred from the fact that a section of the ten-mile crust having the surface area of the United States, represents only about 1.5 per cent. of the entire mass of matter under consideration. Even the mass of Lake Superior thus bcomes a negligible quantity.

The composition of the ocean is easily determined from the data

[1] Sir John Murray, *Scottish Geograph. Mag.*, 1888, p. 39, estimates the volume of the ocean at 323,722,150 cubic miles. Karstens, more recently, puts it at 1,285,935,211 cubic kilometres, or 307,496,000 cubic miles. "Eine neue Berechnung der mittleren Tiefen der Oceane," *Inaug. Diss.*, Kiel, 1894. Karstens gives a good summary of previous estimates, which vary widely. To change the figure given in my original essay would be a straining after unattainable precision.

The adoption of Murray's figure for the volume of the ocean would raise its percentage to from 7.12 to 7.88, according to the density, 2.5 or 2.7, assigned to the lithosphere.

given by Dittmar in the Report of the Challenger Expedition.[1] The maximum salinity observed by him amounted to 37.37 grammes of salts in a kilogramme of water, and by taking this figure instead of a lower average value, we can allow for saline masses enclosed within the solid crust of the earth, and which would not otherwise appear in our final estimates. Combining this datum with Dittmar's figures for the average composition of the oceanic salts, we get the second of the subjoined columns. Other elements, contained in sea water, but only in minute traces, need not be considered here. No one of them could reach 0.001 of 1 per cent.

Composition of Salts.		Composition of Ocean.	
$NaCl$	77.76	O	85.79
$MgCl_2$	10.88	H	10.67
$MgSO_4$	4.74	Cl	2.07
$CaSO_4$	3.60	Na	1.14
K_2SO_4	2.46	Mg	.14
$MgBr_2$	.22	Ca	.05
$CaCO_3$	.34	K	.04
	100.00	S	.09
		Br	.008
		C	.002
			100.00

It is worth while at this point to consider how large a mass of matter these oceanic salts represent. The *average* salinity of the ocean is not far from 3.5 per cent.; its mean density is 1.027, and its volume is 302,000,000 cubic miles. The specific gravity of the salts, as nearly as can be computed, is 2.25. From these data it can be shown that the volume of the saline matter in the ocean is a little over 4,800,000 cubic miles, or enough to cover the entire surface of the United States, excluding Alaska, 1.6 miles deep. In face of these figures, the beds of rock salt at Stassfurt and elsewhere, which seem so enormous at close range, become absolutely trivial. The allowance made for them by using the maximum salinity of the ocean instead of the average, is more than sufficient; for it gives them a total volume of 325,000 cubic miles. That is, the data used for computing the average composition of the ocean, and its average

[1] In Volume 1, "Physics and Chemistry."

significance as a part of all terrestrial matter, are maxima; and tend therefore to compensate for the omission of factors which could not well be estimated directly.

The average composition of the lithosphere is very nearly that of the igneous rocks alone. The sedimentary rocks represent altered igneous material, from which salts have been leached into the ocean, and to which oxygen, water and carbon dioxide have been added from the atmosphere. For these changes, corrections can be applied, and their magnitude and effect, as will be shown later, is surprisingly small. The thin film of organic matter upon the surface of the earth can be neglected altogether. In comparison with the ten-mile thickness of rock below it, its quantity is too small to be considered. Even beds of coal are negligible, for their volume also is relatively insignificant. Practically, we have to consider at first, only ten miles of igneous rock; which, when large enough areas are studied, averages much alike in composition all over the globe. This point was established in my original memoir, in which groups of analyses, representing rocks from different regions, were compared. The essential uniformity of the averages was unmistakable, and it has been still further emphasized by later computations by others as well as by myself. The following averages are now available for comparison.

A. My original average of 880 analyses, of which 207 were made in the laboratory of the U. S. Geological Survey and 673 were collected from other sources. Many of these analyses were incomplete.

B. The average of 680 analyses from the records of the Survey laboratories, plus some hundreds of determinations of silica, lime and alkalies. The Survey data up to January 1, 1897.

C. The average of 830 analyses from the Survey records, plus some partial determinations. The Survey data up to January 1, 1900.

D. An average of all the analyses, partial or complete, made up to January 1, 1904, in the laboratories of the Survey.[1]

E. An average, computed by A. Harker,[2] of 397 analyses of

[1] See U. S. Geological Surv. Bull. 228, p. 17, for details.

[2] *Geol. Mag.*, 1899, p. 220.

PROC. AMER. PHIL. SOC., XLV. 182B, PRINTED JUNE 25, 1906.

igneous rocks from British localities. Many of these analyses were incomplete, especially with respect to phosphorus and titanium.

F. An average of 1,811 analyses, from Washington's tables.[1] Calculated by H. S. Washington. The data represent material from all parts of the world.

Now, omitting minor constituents, which rarely appear except in the more modern analyses, these averages may be tabulated together, although they are not absolutely comparable. The comparison assumes the following form:

	A. Clarke.	B. Clarke.	C. Clarke.	D. Clarke.	E. Harker.	F. Washington.
SiO_2	58.59	59.77	59.71	60.91	58.75	58.239
Al_2O_3	15.04	15.38	15.41	15.28	15.64	15.796
Fe_2O_3	3.94	2.65	2.63	2.63	5.34	3.334
FeO	3.48	3.44	3.52	3.46	2.40	3.874
MgO	4.49	4.40	4.36	4.13	4.09	3.843
CaO	5.29	4.81	4.90	4.88	4.98	5.221
Na_2O	3.20	3.61	3.55	3.45	3.25	3.912
K_2O	2.90	2.83	2.80	2.98	2.74	3.161
H_2O at 100°	1.96	.....	.....	.41	2.23	.363
H_2O above 100°		1.51	1.52	1.49		1.428
TiO_2	.55	.53	.60	.73	.12	1.039
P_2O_5	.22	.21	.22	.26	.02	.373
	99.66	99.14	99.22	100.61	99.56	100.583

Although these six columns are not very divergent, there are differences between them which may be more apparent than real. Differences of summation are partly due to the omission of minor constituents; but the largest variations are attributable to the water. In two columns hygroscopic water is omitted; in two it is not distinguished from combined water; in two a discrimination is made. By rejecting the figures for water and recalculating to 100 per cent., the averages become more nearly alike, as follows:

[1] U. S. Geological Surv. Professional Paper 14, p. 106.

	A. Clarke.	B. Clarke.	C. Clarke.	D. Clarke.	E. Harker.	F. Washington.
SiO_2	59.97	61.22	61.12	61.71	60.36	58.96
Al_2O_3	15.39	15.75	15.77	15.48	16.07	15.99
Fe_2O_3	4.03	2.71	2.69	2.68	5.48	3.37
FeO	3.56	3.53	3.60	3.50	2.46	3.93
MgO	4.60	4.51	4.46	4.18	4.20	3.89
CaO	5.41	4.93	5.02	4.94	5.12	5.28
Na_2O	3.28	3.69	3.63	3.49	3.34	3.96
K_2O	2.97	2.90	2.87	3.02	2.83	3.20
TiO_2	.56	.54	.61	.74	.12	1.05
P_2O_5	.23	.22	.23	.26	.02	.37
	100.00	100.00	100.00	100.00	100.00	100.00

Of these averages, only "D" and "F" need be considered any further, for they include the largest masses of trustworthy data. "A" was only a preliminary computation, "B" and "C" are included under "D"; Harker's average contains too many imperfect analyses. "D" and "F," however, are not strictly equivalent. Washington's average relates only to analyses which were nominally complete, and made in many laboratories by very diverse methods. My average represents the homogeneous work of one laboratory, and includes, moreover, many partial determinations. For the simpler salic rocks determinations of silica, lime and alkalies are generally all that is needed for petrographic purposes. The femic rocks are mineralogically more complex, and for them full analyses are necessary. The partial analyses, therefore, chiefly represent salic rocks, and their inclusion in the average tends to raise the percentage of silica and to lower the proportions of other elements. The salic rocks, however, are more abundant than those of the other class, and so the higher figure for silica seems more probable. This conclusion is in line with a criticism by Mennell,[1] who thinks that the femic rocks received excessive weight in my earlier averages. Mennell has studied the rocks of southern Africa, where granitic types are predominant; and he believes that the true average should approximate to the composition of a granite. A wider range of

[1] *Geol Mag.*, ser. 5, vol. 1, p. 263. For other discussions of the data given in my former papers, see De Launay, *Revue Gén. des Sciences*, April 30, 1904; and Ochsenius, *Zeitsch, prakt. Geol.*, May, 1898.

observation would probably modify that opinion, which, however, is entitled to some weight.

So far, my final average has only been partly given; the minor constituents of the rocks remain to be taken into account. In the laboratory of the Geological Survey the analyses of igneous rocks have been unusually elaborate, and many things have been determined which are too often ignored. The complete average is given in the next table, with the number of determinations to which each figure corresponds. In the elementary column, hygroscopic water does not appear; but an allowance is made for a small amount of iron which was reported in the analysis as FeS_2. When a "trace" of anything is recorded, it is arbitrarily reckoned as 0.01 per cent., and when a substance is known to be absent from a rock, by actual determination of the fact, it is assigned zero value in making up the averages.

	Number.	Average.	Reduced to 100.	In Elementary Form	
SiO_2	1,358	60.91	59.87	O	47.09
Al_2O_3	912	15.28	15.02	Si	28.23
Fe_2O_3	961	2.63	2.58	Al	7.99
FeO	962	3.46	3.40	Fe	4.46
MgO	1,027	4.13	4.06	Mg	2.46
CaO	1,215	4.88	4.79	Ca	3.43
Na_2O	1,268	3.45	3.39	Na	2.53
K_2O	1,265	2.98	2.93	K	2.44
H_2O-	770	.41	.40	H	.17
H_2O+	832	1.49	1.46	Ti	.43
TiO_2	870	.73	.72	Zr	.026
ZrO_2	185	.03	.03	C	.14
CO_2	469	.53	.52	P	.11
P_2O_5	884	.26	.26	S	.11
S	575	.11	.11	Cl	.07
Cl	234	.07	.07	F	.02
F	73	.02	.02	Ba	.089
BaO	617	.11	.11	Sr	.034
SrO	520	.04	.04	Mn	.084
MnO	899	.10	.10	Ni	.023
NiO	243	.03	.03	Cr	.034
Cr_2O_3	246	.05	.05	V	.02
V_2O_3	40	.03	.03	Li	.01
Li_2O	550	.01	.01		100.000
		101.74	100.00		

In this computation the figures for C, Zr, Cl, F, Ni, Cr and V are only very rough approximations. They show, however, that these elements exist in igneous rocks in determinable quantities. The elements not included in the calculation represent minor corrections, to be applied whenever the necessity for doing so may arise. For estimates of their probable amounts, the papers by Vogt[1] and Kemp[2] can be consulted. It is probable that no one of them would reach 0.01 per cent. The elements not mentioned in the table cannot amount to more than 0.5 per cent. altogether; and even that small figure is likely to be an overestimate.

	Shale. A.	Sandstone. B.	Limestone. C.
SiO_2	58.38	78.66	5.19
Al_2O_3	15.47	4.78	.81
Fe_2O_3	4.03	1.08	} .54
FeO	2.46	.30	
MgO	2.45	1.17	7.90
CaO	3.12	5.52	42.61
Na_2O	1.31	.45	.05
K_2O	3.25	1.32	.33
H_2O at 110°	1.34	.31	.21
H_2O above 110°	3.68	1.33[3]	.56[3]
TiO_2	.65	.25	.06
CO_2	2.64	5.04	41.58
P_2O_5	.17	.08	.04
S	.....	.....	.09
SO_3	.65	.07	.05
Cl	.....	trace	.02
BaO	.05	.05	none
SrO	none	none	none
MnO	trace	trace	.05
Li_2O	trace	trace	trace
C, organic	.81	.....	.....
	100.46	100.41	100.09

Before we can finally determine the composition of the lithosphere, the sedimentary rocks must be taken into account; and to do this we must ascertain their relative quantity. First, however, we may consider their composition, which has been determined by

[1] *Zeitsch. prakt. Geol.*, 1898, pp. 225, 314, 377, 413, and 1899, pp. 10, 274.

[2] *Science*, Jan. 5, 1906, and *Economic Geology*, vol. 1, pp. 207.

[3] Includes organic matter.

means of composite analyses. That is, instead of averaging analyses, average mixtures of many rocks were prepared,[1] and these were analyzed once for all. The results appear in the preceding table.

A. Composite analyses of 78 shales; or more strictly, the average of two smaller composites, properly weighted.

B. Composite analysis of 253 sandstones.

C. Composite analysis of 345 limestones.

In attempting to compare these analyses with the average composition of the igneous rocks, we must remember that they do not represent definite substances, but mixtures shading into one another. The average limestone contains some clay and sand, the average shale contains some calcium carbonate. Furthermore, they do not cover all of the products derived from the decomposition of the primitive rock, for the great masses of sediments on the bottom of the ocean are left out of account. The analyses of the latter are too few to give conclusive averages, but the data published in the Challenger Reports[2] indicate a difference between them and the terrigenous deposits. The "red clay," for example, which covers 51,500,000 miles of the ocean floor, at its greatest depths, is much richer in iron than the average shale. In twenty-one analyses of this sediment a mean of 13.61 per cent. of ferric oxide was found. The thickness of this deposit is quite unknown. There are also metamorphic rocks to be considered, such as amphibolites and serpentines; although their quantities are presumably too small to seriously modify the final averages. They might, however, help to explain a deficiency of magnesium which appears in the sedimentary analyses. Partly on account of these considerations, and partly because the sedimentary rocks contain water and carbon dioxide which have been added to the original igneous material, we cannot recombine the composite analyses so as to exactly reproduce the composition of the primitive matter. To do this it would be necessary also to allow for the oceanic salts, which represent, in part at least, losses from the land; but that factor in the problem is perhaps the least

[1] These mixtures were prepared by G. W. Stose, under the direction of G. K. Gilbert. The analyses were made by H. N. Stokes in the laboratory of the U. S. Geological Survey. See Bull. 228, p. 20.

[2] "Volume on Deep Sea Deposits," p. 198.

embarrassing. Its magnitude is easily estimated, and it gives a measure of the extent to which the igneous rocks have been decomposed.

If we assume that all of the sodium in the ocean was derived from the leaching of the primitive rocks, and that the average composition of the latter is correct, it is easy to show that the marine portion is very nearly one thirtieth of that contained in the ten-mile lithosphere. That is, the complete decomposition of a shell of igneous rock, one third of a mile thick, would yield all the sodium in the ocean. Some sodium, however, is retained by the sediments, and the analyses show that it is about one third of the total amount. That is, the oceanic sodium represents two thirds of the decomposition, and the estimate must, therefore, be increased one half. On this basis, a rocky shell one half mile thick, completely enveloping the globe, would slightly exceed the amount needed to furnish the sodium of the sea and the sediments. No probable change in the composition of the lithosphere can modify this estimate very considerably; and since the ocean may contain primitive sodium, not derived from the rocks, the half mile must be regarded as a maximum allowance. If the primeval rocks were richer in sodium than those of the present day, a smaller mass of them would suffice; if poorer, more would be needed to account for the salt in the sea. Of the two suppositions, the former is the more probable; but neither assumption is necessary. If, however, we assume that our igneous rocks are not altogether primary, but that some of them represent re-fused or metamorphosed sedimentaries, we must conclude that they have been partly leached and have, therefore lost sodium. That is, the original matter was richer in sodium, and the half-mile estimate is consequently too large.

From another point of view, the thinness of the sediments can be simply illustrated. The superficial area of the earth is 199,712,000 square miles, of which 55,000,000 are land. According to Geikie,[1] the mean elevation of all the continents is 2,411 feet. Hence, if all of the land now above sea level were spread uniformly over the globe, it would form a shell about 660 feet thick. If we

[1] "Textbook of Geology," 4th ed., vol. 1, p. 49.

assume this matter to be all sedimentary, which it certainly is not, and add to it any probable allowance for the sediments at the bottom of the sea, we shall still fall far short of the half-mile shell, which, on chemical evidence, is a maximum. In the following calculation this maximum will be taken for granted.

The relative proportions of the different sedimentary rocks within the half-mile shell can only be estimated approximately. Such an estimate is best made by studying the average igneous rock, and determining in what way it can break down. A statistical examination of about 700 igneous rocks, which had been described petrographically, leads to the following rough estimate of their mean mineralogical composition:

Quartz	12.0
Feldspars	59.5
Hornblende and pyroxene	16.8
Mica	3.8
Accessory minerals	7.9
	100.0

The average limestone contains 76 per cent. of calcium carbonate, and the composite analyses of shales and sandstones correspond to the subjoined percentages of component minerals:

	Shale.	Sandstone.
Quartz[1]	22.3	66.8
Feldspars	30.0	11.5
Clay[2]	25.0	6.6
Limonite	5.6	1.8
Carbonates	5.7	11.1
Other minerals	11.4	2.2
	100.0	100.0

If now we assume that all of the igneous quartz, 12 per cent., has become sandstone, it will yield 18 per cent. of that rock, which is evidently a maximum. Some quartz has remained in the shales. One hundred parts of the average igneous rock will form, on decomposition, less than 18 parts of sandstone.

The igneous rocks contain, as shown in the last analysis cited,

[1] The total percentage of free silica.

[2] Probably sericite in part. In that case the feldspar figure becomes lower.

4.79 per cent. of lime. This would form 8.55 per cent. of calcium carbonate, or 11.2 per cent. of the average limestone. But at least half of the lime has remained in the other sediments, so that its true proportion can not reach 6 per cent., or one third the proportion of the sandstones. The remainder of the igneous material, plus some water and minus oceanic sodium, has formed the siliceous residues which are grouped under the vague title of shale. Broadly, then, we may estimate that the lithosphere, within the limits assumed in this memoir, contains 95 per cent. of igneous rock, with 5 per cent. of sedimentaries. If we assign 4.0 per cent. to the shales, 0.75 per cent. to the sandstones, and 0.25 per cent. to the limestones, we shall come as near to the truth as is possible with the present data.[1] On this basis, the average composition of the lithosphere may be summed up as follows. The analyses of the sedimentary rocks are recalculated to 100 per cent.:

	95 Per Cent. Igneous.	4 Per Cent. Shale	0.75 Per Cent. Sandstone	0.25 Per Cent. Limestone.	Average.
SiO_2	59.87	58.10	78.33	5.19	59.79
Al_2O_3	15.02	15.40	4.77	.81	14.92
Fe_2O_3	2.58	4.02	1.07	.54	2.63
FeO	3.40	2.45	.30	....	3.33
MgO	4.06	2.44	1.16	7.89	3.98
CaO	4.79	3.11	5.50	42.57	4.82
Na_2O	3.39	1.30	.45	.05	3.28
K_2O	2.93	3.24	1.31	.33	2.96
H_2O	1.86	5.00	1.63	.77	1.98
TiO_2	.72	.65	.25	.06	.71
ZrO_2	.03	....	....	....	.03
CO_2	.52	2.63	5.03	41.54	.74
P_2O_5	.26	.17	.08	.04	.25
S	.11	....	....	.09	.10
SO_3	....	.64	.07	.05	.02
Cl	.07	....	....	.02	.07
F	.02	....	....	....	.02
BaO	.11	.05	.05	....	.10
SrO	.04	....	....	....	.04
MnO	.10	....	....	.05	.09
NiO	.03	....	....	....	.03
Cr_2O_3	.05	....	....	....	.05
V_2O_3	.03	....	....	....	.02
Li_2O	.01	....	....	....	.01
C	....	.80	....	....	.03
	100.00	100.00	100.00	100.00	100.00

[1] Van Hise, "A Treatise on Metamorphism," U. S. G. S. Monograph 47, p. 940, divides the sedimentary rocks into 65 per cent. shales, including all pelites and psephites, 30 per cent. sandstones, and 5 per cent. limestones.

The final average differs from that of the igneous rocks alone, only within the limits of uncertainty due to experimental errors, and the assumptions made as to the relative proportions of the sedimentaries. If the work were ideally exact, the last column of figures should differ from the first symmetrically, being higher in water and carbon dioxide and lower in all other constituents. Lime and potash, however, show small gains, which are abnormal, and indicative to some extent of the errors above mentioned. It is possible that excessive weight has been assigned to the limestones, but on that theme it is hardly worth while to speculate. The values chosen for the sediments are approximations only, and nothing more can be claimed for them. They seem to be near the truth; as near as we can approach with data which are necessarily imperfect, and so they may be allowed to stand without further emendation.

Now, with the help of this new average, we are in a position to compute the relative abundance of the chemical elements in all known terrestrial matter. For this purpose, the column is restated in elementary form, with an arbitrary allowance of 0.5 per cent. for all of the elements not specifically included in it. As for the atmosphere, it is represented in the final result by 0.02 per cent. of nitrogen, which is a little too low. The mean composition of the lithosphere, the ocean and the atmosphere, then, is as follows:

	93 Per Cent. Lithosphere.	7 Per Cent Ocean	Average, Including Nitrogen.
Oxygen	47.07	85.79	49.77
Silicon	28.06	....	26.08
Aluminum	7.90	....	7.34
Iron	4.43	....	4.11
Calcium	3.44	.05	3.19
Magnesium	2.40	.14	2.24
Sodium	2.43	1.14	2.33
Potassium	2.45	.04	2.28
Hydrogen	.22	10.67	.95
Titanium	.40	....	.39
Carbon	.20	.002	.18
Chlorine	.07	2.07	.21
Bromine	....	.008	....
Phosphorus	.11	....	.10
Sulphur	.11	.09	.10
Barium	.09	....	.09
Manganese	.07	....	.07
Strontium	.03	....	.03
Nitrogen	....	....	.02
Fluorine	.02	....	.02
All other elements.	.50	....	.50
	100.00	100.00	100.00

It would be foolish to ascribe any high degree of accuracy to these figures, for the data are confessedly of very unequal value. They do show, however, clearly and conclusively, the order of magnitudes with which we have to deal. We may claim to know, for example, that oxygen forms about one half of all known terrestrial matter and silicon about one fourth. Next in order of abundance comes aluminum, then iron, and then calcium, followed by magnesium, sodium and potassium in nearly equal proportions. So much is established, and we are also able to say that certain other elements appear in minor, but determinable, amounts. In its general drift, the table is satisfactory; but its details are subject to revision. The question now is, what legitimate uses can be made of it? What problems can it help us to solve?

My answers to these questions, I fear, can hardly be satisfactory. The more closely I scrutinize the uses which have been made of the averages, the more questionable I find them to be. For instance, it is possible to compute from the average chemical or mineralogical composition of the igneous rocks their average physical properties; and as the component data in such a calculation are usually quantities of similar magnitude, the results obtained will probably be quite near the truth. Mr. W. H. Emmons,[1] for example, has determined, from one of my averages, the average mineralogical composition to which it corresponds, using for that purpose the norms of the new quantitative classification. Then, from the known coefficients of expansion of the minerals, he has calculated the mean coefficient for the igneous crust of the earth. The value found is 0.0000199, and its uncertainty cannot be very large. But, after all, what can be done with the figure? If we use it to discuss the swellings and shrinkings of the lithosphere, we are limited to surface phenomena alone, in which the disturbances due to cracks and crevices in the rocks are exceedingly large. If we go below the surface, to a point where the rocks are presumably continuous, this particular coefficient of expansion ceases to be applicable, for it has been modified by changes in temperature and by increased pressure. In other words, our constant is only a constant under

[1] In Chamberlin and Salisbury's "Geology," vol. 1, p. 546.

surface conditions, and at five or ten miles below the surface it must assume a different and unknown value. The coefficient becomes larger as the temperature rises, but the influence of increased pressure is undetermined. In short, the data as they stand to-day are inapplicable to discussions of this kind; although it is conceivable that future discoveries may enable us to eliminate the difficulties that now exist. When the physical properties of rocks and minerals shall have been measured under widely differing conditions of temperature and pressure, we may be able to apply the data to such averages as I have given; and so assist in the solution of geophysical problems.

Professor Van Hise[1] has attempted to combine my third average for the igneous rocks with the composite analyses of the sedimentaries, in order to determine the redistribution of the elements during the processes of metamorphism. In so doing he has assumed that shales, sandstones and limestones exist respectively in the proportions of 65, 30 and 5 per cent. of the sedimentary rocks; and upon recombining the data with allowances for matter contained in the ocean, he found various excesses and deficiencies. These differences from the parent rock he seeks to explain; but it seems to me that his efforts are premature. The data are not yet sufficiently precise to justify so elaborate a discussion, as a comparison of Mr. Washington's average with mine will show. Furthermore, as I have already observed, several important factors in the problem remain to be determined. The metamorphosed sediments and the oceanic deposits are not covered by the composite analyses, and that relative to the shales is otherwise imperfect. It represents only 78 rocks, a number which is quite inadequate. A much larger amount of more varied material must be studied before the analyses of the igneous and sedimentary rocks can be well fitted together. Only the broadest relations are ascertainable now, as I have sought to show in the preceding pages. We can determine, roughly, the *maximum* relative mass of the sedimentaries, and also something of their proportional quantities; but much farther than that we are hardly yet ready to go. One suggestion, however, merely as a sug-

[1] "A Treatise on Metamorphism," pp. 947–1002.

gestion, may be worth considering. The quantity of carbon dioxide locked up within the lithosphere is, as shown by the data now before us, about equal to twenty-five times the mass of the atmosphere. To that quantity must be added at least three more atmospheres, and perhaps a much larger quantity of oxygen which has been consumed in transforming the ferrous compounds of the igneous rocks into the ferric oxide of the sediments. We may well ask whether all of this material was actually absorbed from the atmosphere, and if so, whence was it derived? Did the primeval atmosphere contain it all at once, or was it drawn from cosmical sources, or expelled first from the earth's interior? These questions I shall not attempt to answer, for I do not care to enter the realm of speculation. It is enough for me to point out the order of the quantities involved in the problems thus suggested, problems which have been the themes of many writers, and leave them as crude data for possible future use.

One other phase of geochemical statistics remains to be mentioned; namely, the attempt to use chemical evidence in the measurement of geological time. At least two such efforts have been made; the one by T. Mellard Reade,[1] the other by Professor Joly.[2] Mr. Reade, from a study of the soluble substances contained in the surface waters of England and Wales, estimates that their removal from the rocks and soil would lower the level of those countries at the rate of one foot in 12,978 years. This means a transfer to the ocean of dissolved matter alone equivalent to 143.5 tons per annum from each square mile of land; and to this must be added the solid sediments. From various data relative to the drainage basins of Europe, and to some large rivers in other parts of the world, Reade calculates that the average denudation of all the land of the globe amounts to about 800 tons annually per square mile. This figure, combined with the supposition that the sediments represent a thickness of ten miles, gives a period of 526,000,000 years since the process of sedimentation began. The assumed thickness is evidently many times too great, and the figure is, therefore, excessive.

[1] "Chemical Denudation in Relation to Geological Time," London, 1879.

[2] *Sci. Trans. Royal Dublin Soc.*, ser. 2, vol 7, p. 23. Alsa a note in *Chem. News*, vol. 83, p. 301.

In another calculation, Reade has estimated that the limestones are equal in bulk to a zone 528 feet thick completely enveloping the globe. Then, from the rate at which lime salts are carried from the land into the sea, he computes the time required to form the limestones as equal to 600,000,000 years. I may note here, in passing, that if my own estimate of the mass of the limestones is correct, this quantity should be divided by four, giving 150,000,000 years as the time needed for their formation. I do not care now to criticize Reade's calculations in detail, for his data were in many respects defective, and the conjectural element in his reasoning was very large. In spite of these obvious objections, however, Reade's work has shown certain statistical possibilities, and has pointed out a line of investigation that may be profitably followed. Previous to the appearance of Reade's memoir the mechanical sediments had been used in estimating geological time, but the dissolved matter, which is of nearly equal importance and much more easily measurable, was neglected.

Professor Joly's line of attack upon the time problem was analagous to Reade's, but different. Taking as his fundamental datum the quantity of sodium in the ocean, and then estimating the annual amount of sodium brought in by waters from the land, he was able to compute the time required for the oceanic accumulation. This method of calculation is simple, direct and clear, provided the rate of supply has been constant, and that all corrections are known and applied. Uncorrected, the time needed for the observed accumulation is 99,400,000 years, which is evidently a maximum. Corrected, by a questionable allowance for pre-sedimentary sodium in the ocean, and for cyclic salts lifted by winds from the sea and returned to it again, the estimate is reduced to 89,300,000 years. If we admit that Joly's data are correct, we may round off his figures to between ninety and one hundred millions of years, and feel reasonably confident that the time of sedimentation is a quantity of that order.[1]

There are two weak points in Joly's calculation. First, the mag-

[1] For discussions of Joly's memoir see Mackie, *Trans. Edin. Geol. Soc.*, vol. 8, p. 240; Fisher, *Geol. Mag.*, 1900, p. 124; Ackroyd, *Chem. News*, vol. 83, p. 265; 84, p. 56; and *Geol. Mag.*, Aug. and Oct. 1901. See also Sollas, "The Age of the Earth," p. 21.

nitudes of his corrections are uncertain, and secondly, our knowledge of the contribution made by rivers to the sea is most imperfect. Joly has used Sir John Murray's estimate of the composition of river water,[1] and that rests upon insufficient data. Murray has averaged together the analyses of nineteen rivers, which are not named, but which were presumably, for the most part, European. Data are lacking for the great African and Asiatic rivers, the Nile excepted, and the other available material is incomplete. A river varies in composition from time to time and from place to place; so that a single analysis of it may be misleading in the highest degree. And yet many of the published analyses are of that character; that is, they represent single samples of water from river systems in which the local and annual variations may be very large. Furthermore, Murray's average is mainly, if not wholly, of waters from the temperate zone; from which, in all probability, tropical and arctic waters may differ considerably. I speak thus advisedly, for I have compared, and reduced to uniform standards, more than a hundred analyses of river waters, and have noted their wide variations. For a very few streams the average annual composition is known, and from such averages, weighted in accordance with the areas drained, the final estimate must be made. The data are now being gathered; the nature of the work to be done is well understood, and in a few years it may be possible to replace Murray's average with one of a more definite character. Then, and not till then, can Joly's method be profitably applied to the discussion of geological time. His results may or may not be seriously modified, but the conclusions reached will be more definite than any we can attain to now. At all events, the present rate of chemical denudation is a measurable quantity, and it will form an important statistical datum for use in the investigation of various problems.

In conclusion I may be permitted to urge upon chemists and geologists the importance of the statistical method in the investigation of large geochemical problems. The method is evidently applicable in many cases, and leads to conclusions of positive value. We need, however, better material to work with than we have now,

[1] *Scottish Geographical Magazine*, 1887, p. 76.

and I have indicated some of the desiderata. From chemical evidence we can draw deductions relative to the volumes and masses of the sediments, and also gain something towards the measurement of geological time. Even if the conclusions to be reached by these methods are not final, they are at least helpful. In order to be convincing, the chemical evidence must follow lines convergent with other testimony, and when it does so we may be satisfied that we are approaching the truth.

ON A POSSIBLE REVERSAL OF DEEP-SEA CIRCULATION AND ITS INFLUENCE ON GEOLOGIC CLIMATES.[1]

By THOMAS C. CHAMBERLIN.

(*Read April 18, 1906.*)

Among the multitude of subjects that drew illuminating thought from the cosmopolitan philosopher to whom we pay our homage, were the phenomena of the atmosphere and the ocean. Aside from atmspheric electricity, certain climatic phenomena were subjects of his special study, as we are to learn more fully in the course of these memorial exercises. But Benjamin Franklin was above all a student of human affairs and his physical inquiries were instinctively correlated with human interests. If, therefore, in treating a phase of oceanic circulation and its bearings on geologic climates, I associate my subject frankly with the interests of our race, I trust it may be assigned to a desire to bring my contribution into harmony with the spirit of the distinguished American philosopher of the eighteenth century.

The control of secular climates is obviously a condition prerequisite to biologic continuity. The preservation of a narrow range of temperature and a limited variation of atmospheric constituents throughout the millions of years of the biologic past was absolutely essential to organic evolution. Continued preservation for millions of years to come seems equally a condition precedent to an intellectual and spiritual evolution commensurate with the physical and biological evolutions that have preceded it. Only such a prolonged evolution of the intellectuality now just dawning gives full moral satisfaction to our conception of the sum-total of terrestrial history.

The narrowness of the range to which temperatures must be confined to permit progressive organic and intellectual evolution takes on its true meaning only when we recall that the natural tem-

[1] Presented by permission of the President of the Carnegie Institution under whose auspices these studies have been prosecuted.

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perature-range on the earth's surface is sixteen times as great as this, while that affecting the solar family is at least sixty times as great. For a hundred million years, more or less, this narrow range of temperature has been maintained quite without break of continuity, unless geologists and biologists are altogether in error in their inductions. On the further maintenance of this continuity hang future interests of transcendent moment.

So too the maintenance of a narrow range of atmospheric constitution, notably in the critical element carbon dioxide, has been equally indispensable. These two critical limitations of temperature and of constitution seem also to have been interdependently correlated with one another.

The climatic problem is as difficult as it is important. The factors are so many, so elusive, so imperfectly determined, perhaps even so imperfectly determinable, that the utmost patience and assiduity are a duty of the investigator, and the utmost charity of judgment an obligation of fellow scientists. I am persuaded, however, that tentative analyses of the tangle of factors are an indispensable aid to the future solution of the problem. One of the gravest difficulties confronting us to-day is the imperfection of observations and the inconclusiveness of experimentation; and this arises in no small degree from the lack of such patient preliminary analyses of the problem as shall bring into sharp recognition the occult things that are to be observed and the precise experimental determinations which alone can really aid in the solution. If the little contribution of this half hour shall have any value at all, it will lie in its suggestive relations to the larger problem of secular climates, past and prospective.

As this larger problem has recently assumed, with some of us, a phase much at variance with its more familiar aspects, it may need to be briefly sketched. It has been customary to assign to the primitive earth a climate quite beyond the Miltonian conception of Gehenna in its fiery intensity, and to predict an impending refrigeration scarcely inferior in antithetic supremacy. The familiar conception of the sum-total of atmospheric history as a decline from one excess to another as the sequence of thermal wastage, is a logical deduction from the hypothetical derivation of the earth from

a gaseous or quasi-gaseous nebula through gravitative condensation. To some of us, however, such a derivation seems inconsistent with the dynamics of the present solar system, and an alternative hypothesis has been formulated to meet the supposed requirements of existing phenomena. The acceptance of this requires a reconstruction of the whole conception of geologic climates. The new view discards the primitive molten state as a necessary condition and presents the alternative of a slow growth of the earth by planetesimal accessions. This alternative involves a slow growth of the atmosphere also, until it reached a volume similar to the present, when its growth is assumed to have been arrested and thereafter limited by the interplay of opposing agencies. These agencies are thought to have held it ever since within so narrow a range of oscillation as to foster organic evolution. A continuance of the same control offers ground for hope of a perpetuation of conditions congenial to organic and intellectual life, through a period to which no definite limits can now be set beyond the presumption that there must ultimately be a limit. The inevitable cooling of a once white-hot earth plays no part in this prognosis. The agencies of atmospheric *maintenance* and *control* thus force themselves upon consideration as factors of supreme importance.

The assigned agencies of atmospheric *restraint* are molecular velocities, chemical combination and condensation. By virtue of the first, the lighter constituents are reduced to a minimum and all constituents are restricted within certain large limits. By virtue of the second, the chemically active factors are kept down to states of dilution compatible with organic evolution, while the inert elements have probably been permitted to increase steadily. By the third, the excess of water-vapor has been condensed into the ocean, which has probably increased rather than diminished through the ages.

The postulated agencies of atmospheric *supply* are accessions from without and emanations from within, of which Vesuvius is just now giving us an impressive illustration.

To the interplay of these opposing agencies of loss and gain is assigned the maintenance of the requisite narrow range of atmospheric constitution, of temperature, and of associated conditions. Under this general resetting of fundamental conceptions, the ques-

tion of climatic regulation takes on very concrete aspects and presents specific lines of study.

Subsidiary to these narrow limitations, the recognition of pronounced variations is forced upon us by a growing mass of geologic evidence. Throughout most of the well-known geologic periods, the poleward distribution of life implies warm climates, even as high as 70° and 80° of latitude. How life of sub-tropical types could have survived the long polar nights is one of the most obdurate puzzles of the earth's climatology. It becomes all the more strenuous if we cast aside all resort to an early fervid state and a molten interior. Quite irrespective of primitive conceptions, however, the edge of the problem has sharpened as we have been forced to recognize that *between* the warm polar stages there were episodes of glaciation in strangely low latitudes. It appears necessary now to accept as demonstrative the evidences of extensive glaciation in India, Australia and South Africa in the midst of the later coal-forming stages of the Paleozoic era. The glacial beds lie even between coal beds of Permian or Permo-Carboniferous age; while, strangely enough, the areas of glaciation approach and even overlap the tropics of Cancer and Capricorn. And yet, figs and magnolias have grown in Greenland since, and mild polar climates are as well authenticated after as before this climateric glaciation. Less complete evidences from China[1] and Norway imply a very much earlier glaciation, falling in the oldest Cambrian or perhaps even pre-Cambrian times.

The climatic student seems therefore compelled to face oscillations within the known geologic periods ranging from sub-tropical congeniality within the polar circles, on the one hand, to glacial conditions in low latitudes, on the other, and these *in alternating succession:* while neither of these oscillations was permitted to swing across the narrow limital lines of organic endurance. There is little doubt that the ocean, the daughter of the atmosphere, is one of the most potential agencies in controlling these oscillations. It is one of its possible functions in such regulation that invites our present attention.

Some of the regulating functions of the ocean have long been

[1] Willis, "Third Year-Book Carnegie Institution," 1904, p. 282.

recognized. Certain less familiar ones have been brought under study in recent years by a few students independently. Schlœsing was perhaps the first to clearly recognize that the carbon dioxide of the ocean is an important agency in the regulation of the atmospheric content of this critical factor. As early as 1880[1] he advanced the view that the carbon dioxide of the atmosphere is in equilibrium not only with the free carbon dioxide absorbed in the sea water but through dissociation with the second equivalent of carbon dioxide in the oceanic bicarbonates. The sum-total of such free and loosely combined carbon dioxide available at present as a possible supply for the atmosphere may be some twenty-five times the present atmospheric content. Schlœsing held that any depletion of the atmospheric content would be followed by emanation from the ocean, and any excess acquired by the atmosphere would be followed by oceanic absorption, and hence great changes in the atmospheric content would only be brought about by reducing or increasing the large sum-total of atmospheric and oceanic supply. This was a contribution of the first order to the problem of atmospheric regulation. It is necessary for a geologist, however, to recognize that the exchange, and even the equilibrium itself, are dependent on geological and physical conditions. At periods in which the oceanic bicarbonates were most abundant, the amount of free and loose carbon-dioxide in the ocean may perhaps have reached thirty or forty times the present atmospheric content, while on the other hand it may have fallen to a very low figure when the ocean was depleted of carbonates. It is necessary also to recognize that the diffusion of gases in water, so far as it is covered by experiment, is a slow process, and computation seems to show that the supply of carbon dioxide to the atmosphere might be much too slow to offset its consumption under certain geologic conditions, unless effectively aided by oceanic circulation. The active superficial circulation immediately assignable to the winds would aid somewhat but its competency is limited. It was in an attempt to determine the functions of the deep-sea circulation in this interchange that the conceptions of this paper arose.

[1] "Sur la constance de la proportion d'acide carbonique dans l'air," *Comp. Rend.*, 1880, t. 90, p. 1410.

In an endeavor to find some measure of the rate of the abysmal circulation, it became clear that the agencies which influenced the deep-sea movements in opposite phases were very nearly balanced. From this sprang the suggestion that if their relative values were changed to the extent implied by geological evidence there might be a reversal of the direction of the deep-sea circulation and that this might throw light on some of the strange climatic phenomena of the past and give us a new means of forecast of climatic states in the future.

That the deep-sea circulation is now actuated dominantly by polar agencies is clear from the low temperatures of the abysmal waters, even beneath the tropics. It is a firm inference that cold waters creep slowly along the depths from the polar seas equatorward where they gradually rise to the surface and return on more superficial routes. This is not, however, yet a matter of observation and the courses pursued are unknown. It is perhaps more probable that they are gyratory or spiral and complex than that they are simple and direct.

The agencies that affect oceanic circulation include at least: (1) wind, (2) atmospheric transfer, (3) differences of salinity, and (4) differences of temperature, including freezing and thawing. The earth's rotation of course modifies the currents but does not actuate them.

1. The effect of the wind is superficial and familiar, and need only be considered here in so far as it affects the deep-sea circulation. Its currents constitute horizontal circuits, and their frictional effect upon the deep currents is probably slight and of a gyratory phase in the main. In so far as they are strictly horizontal, they doubtless favor equally poleward and equatorward movement in the abysmal waters. If there is a component of their sum-total that favors the piling up of waters in the polar regions, it must favor the present deep circulation. If the opposite is true, it must antagonize it. There seems no way at present to measure the relative amounts of these opposing tendencies. It is plausible enough to reason that the cold air from the polar regions would flow more largely at the base of the atmosphere than would the warmer air from the equatorial regions and that the polar winds would thus

antagonize the present abysmal circulation. But theoretical deductions are rarely sure-footed in these complex subjects. The balance of influence, whatever it may be, is probably so slight as to be negligible.

2. We cannot here attempt to follow empirically the transfers of water by evaporation and precipitation, but general inspection seems to indicate the nature of the average effect. The saturation point of the atmosphere falls progressively from the equator to the poles, and the actual humidity runs roughly parallel to it on the grand average. Poleward movement of the atmosphere leads therefore to a lower content of moisture; equatorward movement to a higher. As the acquisition of moisture lags behind the capacity to hold it, it is a rather firm inference that precipitation exceeds evaporation in the high latitudes and that evaporation exceeds precipitation in the low latitudes, on the grand average. The bearing of observational data is of the same import. The result of these ratios of precipitation and evaporation is a raising of the ocean surface by fresh waters in the polar regions and a lowering of it in the low latitudes accompanied there by concentration of saline constituents. Considered alone and ideally, this should give a slight equatorward gradient and a flow of fresh surface waters in that direction. These fresh waters, however, mingle with the superficial sea waters and involve a movement of these also toward the equator. So far as these affect abysmal movement, they antagonize the present circulation.

3. In so far as evaporation exceeds precipitation in the low latitudes, it results in an increased salinity of the superficial waters and a tendency of these to sink and flow poleward to replace the salt waters carried equatorward by the fresh waters as just observed. If these were the only factors it seems clear that the deep circulation would be poleward.

4. On the other hand, the lower temperatures of the high latitudes increase the density of the water and tend to cause it to sink and flow equatorward. But the low temperatures affect primarily the superficial stratum which is freshened by the superior precipitation of the high latitudes, and both computation and observation show that cold fresher waters may float upon warmer saline waters.

A large part of this cold superficial water flows away in surface currents to lower latitudes.

In view of these complications, the precise mode by which polar agencies control the deep circulation is much less obvious than it might at first seem. There is ground to suspect that the formation and melting of ice is an important factor. In freezing, the salt and gases of the surface layer are largely forced out into the underlying layer. If the surface layer has an average degree of salinity, the underlayer is super-charged, and being also cold, must tend to sink. On the borders of the ice-covered tracts where the precipitation and melting are considerable and where adjacent polar lands pour in much fresh water, the surface layers are so much fresher than the average sea-water that the concentration of salinity by freezing does not overbalance the original freshness. But in those polar regions where there is no inflowage from the land, where precipitation is slight and almost wholly snow, which accumulates on a previously frozen surface and absorbs most of its own summer melting, and where the ice is borne away to lower latitudes and the waters arising from it do not redilute the concentrated waters, it is believed that a sufficient degree of saline concentration, combined with depression of temperature takes place to cause an effective downward movement. This is believed to cooperate with diffusion and conduction in giving the lower body of polar waters the superior gravity which actuates the abysmal circulation. The sea immediately bordering Antarctica and that lying northwest of Greenland seem to furnish these conditions. Moss[1] and Krogh[2] independently have found that at times of northwesterly wind, the air west of Greenland contains about double the usual content of carbon dioxide. This I have suggested may come from waters overcharged with it by the freezing of the overlying layer.

It is not to be inferred, however, that the deep-sea waters derived from the polar regions exceed in salinity the waters of the evaporating tracts of low latitudes, but merely that by this concentration through freezing conjoined with low temperature and modified by

[1] Moss, "Notes on Arctic Air," *Proc. Roy. Dublin Soc.*, Vol. II, 1880.

[2] Krogh, "Abnormal CO_2 Percentage in the Air of Greenland," etc., *Meddelelser om Gronland*, Vol. XXVI, 1804, pp. 409–411.

diffusion and mechanical mixture, water of superior gravity is derived and that this controls the abysmal circulation.

Dr. Otto Pettersson, in an elaborate article, supports by experiment and observation the theory of Bjerknes, that the *melting* of the polar ice also promotes circulation, both superficial and deep-seated, but I can only make reference to this here.[1]

A survey of the existing temperatures and salinities of the ocean also makes it clear that the battle between temperature and salinity is a close one and that no profound change is necessary to turn the balance. The combined results of the many polar expeditions have shown that in the high latitudes of both hemispheres there is a superficial sheet of water two hundred to three hundred meters deep that is colder, but lighter, than that below, because it is fresher. It floats upon a warmer, more saline body of water below. This has been specially demonstrated by the investigations of Nansen.[2] This layer of coldest water moves to lower latitudes superficially in the main, showing that coldness alone is not determinative.

In the open Pacific and Indian oceans hydrostatic equilibrium must be very closely maintained, because of the slight resistance to adjustment. It is shown by the charts of Dr. Alexander Buchan[3] that the concentrated warm saline waters form inverted cone-like masses that reach down some four thousand feet or more. It thus appears that they lie in the same horizons as great masses of colder waters which their salinity must counterbalance. Less striking phenomena of similar import mark the evaporating areas of the north and south Atlantic. The equatorial tracts of freshened waters arising from high precipitation are scarcely traceable to half the depth. This seems to imply that in the low latitudes increased density due to evaporation is more potent than freshening by precipitation, in harmony with theory as already set forth, and that the density due to salinity is not greatly over-matched by the low tempera-

[1] "On the Influence of Ice-melting on Oceanic Circulation," *Geog. Jour.*, XXIV., 1904, pp. 285–333.

[2] "The Norwegian North Polar Expedition, 1893–1896, Scientific Results," Fridjof Nansen, Vol. II, Oceanography of the North Polar Basin.

[3] "Challenger Reports," Summary of Results, Pt. II, Appendix, Rept. on Oceanic Circulation.

ture density of the Antarctic regions from which the Pacific and Indian oceans are not separated by appreciable barriers.

An interesting illustration of the close balance between salinity-density and temperature-density is presented by the saline waters that issue from the Mediterranean in which evaporation is in excess of combined precipitation and inflow from adjacent lands. As a result, the concentrated waters that form the deeper body of the Mediterranean creep out through the bottom section of the Straits of Gibraltar, while the upper section is occupied by a compensating inflow from the Atlantic. Although the straits are shallow, the out-creeping current does not appear in the upper horizons of the adjacent Atlantic waters, according to Buchan's charts, but descends to depths of three thousand to five thousand feet before it finds a horizon of density-equilibrium. It then spreads westerly in a great spatulate wedge across the north Atlantic and occupies the larger part of its area between the depths of four thousand and five thousand feet. (See the maps of Buchan.) It is warmer and more saline than the normal oceanic waters at its horizon, and lies on colder but less saline waters below.

These and similar phenomena point to a notable closeness of the balance between the density effects of salinity and of temperature respectively. More saline but warmer waters both overlie and underlie less saline but colder waters. On the whole, however, at present, the temperature effects are dominant and cold waters occupy the abysmal depths of all the great oceans.

A comparative computation of salinity-effects and of temperature-effects on density, from such data as are now available, leads to a similar conclusion relative to the closeness of balance between the opposing agencies, but this cannot be entered upon here.

Now, as previously remarked, the geological record gives good evidence that in the majority of known periods the temperatures in the polar regions were subtropical or warm temperate. Freezing must apparently have been a trivial factor, if not quite absent, and low temperature was robbed of its chief densifying effects. Evaporation in the zones of descending air currents in low latitudes must apparently have been operative, in some degree at least, to furnish the geological agencies which the record implies. Deposits of salt

and gypsum in not a few periods testify directly to regional aridity. The most marked of these are, to be sure, referable to the periods of glaciation, but many of them have no such assignable association.

In these periods of warm polar temperature there is reason to believe that the high-latitude temperature-effects fell below the low-latitude concentration-effects and that therefore the deep oceanic circulation was actuated by the dense waters of the evaporating tracts. These may then be supposed to have slowly descended and crept poleward, acquiring a trivial amount of heat from the earth's interior and loosing some to the waters above, but substantially maintaining their temperatures until they rose to the surface in the polar regions and gave their warmth to the atmosphere. Aided by the enshrouding mantle of vapors that must have arisen from such a body of water, it is conceived that the mild temperatures requisite for the maintenance of the recorded life through the polar nights may have been thus maintained.

If this be granted, however, it is wise to note that this is not a radical solution of the climatic problem, for a fundamental cause for the conditions that brought on freezing at one period and prevented it at others is prerequisite to the postulated influence of these in the reversal of the abysmal circulation. At the best, our suggestion offers only an auxiliary agency in the control of secular climates. Some more fundamental agency or agencies must be sought.

UNIVERSITY OF CHICAGO,
April 16, 1906.

AN INTERNATIONAL SOUTHERN TELESCOPE.

By EDWARD C. PICKERING.

(*Read April 18, 1906.*)

It is difficult to find a department in the arts or sciences which was not studied by that eminent and practical man, Benjamin Franklin. As his interests were mainly in the practical side of life, it is surprising that one of the least practical subjects, the study of the appearance of the heavenly bodies, should have attracted him. Yet we find that he was probably the first to bring a reflecting telescope to this country. It illustrates the widespread and keen desire of man to probe more and more deeply the sidereal universe. It is also remarkable that the reflector, after falling into disuse for many years, should now appear to be the form of telescope best adapted to this end. The object of the present paper is to propose a practical plan by which a telescope of the largest size should be so constructed and used as to lead to results of the greatest astronomical value.

So careful a study has been made of astronomy, during the last half century, that it is not easy to secure a real advance. We must learn from the success attained in industrial enterprises, and spare no pains to secure the best possible conditions in every respect, however trivial. The best location, the best form, the cost, the method of administration, and the discussion of the results will be considered in turn. It is in the last of these that the greatest advance may be expected. An attempt will be made to show how these results can be discussed, not by an individual or single institution, but by the astronomers of the world, and how numerous departments of astronomy may thus be advanced to a higher plane.

Location.

If we take a map of the world and mark upon it the principal observatories, we shall find that nearly all of them are in locations especially unsuited to good astronomical work. Almost all are near

large cities, capitals of countries, or great universities. These are centers of civilization, since the climate is temperate and frequent rains promote agriculture, inland navigation, and the support of large populations. The very conditions that have rendered man's progress successful are those most unfavorable to good astronomical work. Besides these, smoke, electric lights, and jars, all fatal to the most careful study of the stars, accompany the growth of large cities. If we divide the earth into cloudy and clear halves, nine tenths of the observatories will lie in the cloudy regions.

There are three extensive clear regions upon the earth. The first and largest includes nearly all of the interior of northern Africa. There is no large observatory in that region. The second is in South Africa. The only large observatory there is in Capetown, an exceptional cloudy part of that region. The third region is the interior of Australia. The principal observatories are on the coast, at Melbourne and Sydney.

If we arrange observatories according to latitude, we find that six sevenths of them are between latitudes $+35°$ and $+60°$, or the latitudes of Spain and Scotland. A large part of the southern sky, containing many of the most interesting objects, can never be seen from the observatories of the United States or Europe. If we are to erect the greatest telescope in the world it will have a much wider field of usefulness if placed in the southern hemisphere, where comparatively neglected objects can be studied.

A location should be selected at a considerable elevation, to avoid the dust and haze of the lower atmosphere. These form the greatest obstacles to the use of a large telescope, and their effect is thus reduced to a minimum. In this respect no place is comparable with South America, where one railway attains an elevation of 17,000 feet.

Two locations suggest themselves, the west coast of South America and South Africa. The Harvard College Observatory after careful study, selected a point near Arequipa, Peru. For the last seventeen years it has maintained a station there, at an elevation of 8,000 feet. It is doubtful whether a better location can be found. although it is open to two objections. It is so near the equator that objects near the south pole are always low, and clouds are much more frequent during the summer, from November to March, than

during the remainder of the year. If we go further south, the pole is higher, but the weather is more cloudy. Sir David Gill, Director of the Cape Observatory, recommends Blomfontein. It is one of the most promising locations. It is thirty degrees south of the equator, and the pole is accordingly at that height.

Form of Instrument.

In the time of Franklin, mainly through the triumphs of Sir William Herschel, the reflector was considered the best form of telescope. This form has been frequently used ever since in England, but until recently, it was seldom employed on the continent or in this country. Dr. Henry Draper, nearly half a century ago, recognizing the advantages of the reflector, constructed and used with success one of the largest yet made in this country. One firm, Alvan Clark & Sons, revolutionized public opinion regarding the best form of telescope. The desire to possess the largest telescope in the world has been a common one. There is perhaps no form of memorial which has been more widely known and admired. Five times the Clarks filled an order for the largest telescope in the world, and, in each case except the first, the previous record was their own. They accomplished this by making successively for the Mississippi, Washington, Pulkowa, Lick, and Yerkes Observatories telescopes of 18, 26, 30, 36, and 40 inches aperture. In each case the telescope was a complete success. They proved one of their principles, that whenever they could see an error they could correct it. The limit of size of telescopes of this form seems, however, to be nearly reached. The cost is very great, and the engineering difficulties become serious in the largest instruments.

The genius of one man, the late James E. Keeler, Director of the Lick Observatory, again revolutionized the views of astronomers regarding the advantages of the reflector over the refractor. Having secured a three-foot reflector which previously had done but little work, he obtained with it photographs of extraordinary perfection. Similar results have since been obtained with the two-foot reflector of the Yerkes Observatory. It has thus been shown that, in certain departments of astronomy, especially in photographing faint stars and nebulæ, results could be obtained far beyond those which had

been secured with any form of refractor. One great advantage of the reflector is its low price. The cost of a mirror is about one tenth of that of a lens of the same size. The great defect of large refractors, the color of the images, due to chromatic aberration, is not present in reflectors. The loss of light by absorption increases rapidly with the size of the refractor, and not at all with a reflector. The difference in focus of rays of different colors is so great with a large refractor that the small dispersion needed for photographing the spectra of faint stars cannot be used. No such difference exists with a reflector. On the other hand, the reflector is much more sensitive to changes of temperature or flexure, and the silver surface becomes tarnished and must be renewed at intervals. Nevertheless, in a very large instrument the advantages of a reflector far outweigh those of a refractor.

It is therefore proposed that the telescope shall be a reflector having an aperture of about seven feet, and a focal length of forty-four feet, thus giving images on a scale of 15″ to the millimeter.

Let us imagine the instrument completed, and describe its probable construction. The polar axis is enclosed in an iron cylinder, resembling a boiler, and resting in water according to the method adopted by Mr. Common in the construction of the sixty-inch reflector now at Cambridge. There will be no difficulty from freezing, as the instrument will doubtless eventually be erected in a location having a warm climate. It would be better, if possible, to counterpoise the telescope, taking a large part of the weight off the bearings of the polar axis by a series of ball bearings, if motion of sufficient uniformity can thus be secured. Electric motors furnish abundant power for the motions in right ascension and declination, and a motor controlled by a clock is used for following. This method was employed with entire success in the Harvard telescope, 135 feet long, sent to Jamaica in 1901, and in other telescopes. (See *Astrophysical Journal,* XV, 202.)

The photographic plate is placed at the principal focus of the telescope. For visual work, this is replaced by an inclined mirror which reflects the beam of light to the side of the tube. It then falls upon an astronomical objective of five or six inches aperture, and after undergoing a second reflection, is brought to a focus in

the prolongation of the declination axis of the telescope. It is here enlarged by an eyepiece in the usual way. All objects on the meridian, or at the same hour angle, are thus viewed by the observer without changing his position. He and his recorder are enclosed in a small observing room which protects them from the wind, and which may be warmed if desired. When the object is near the meridian, the observer is looking horizontally, and east or west. As the object moves, the inclination of the line of sight gradually changes, about fifteen degrees an hour. It is probable that the instrument will be used principally for photographic work, and the same method will be employed for following. The possibility of observing a distant object in this way has been established at Harvard, since 1870, with the two eight-inch collimating telescopes of the meridian circle. The images compared in this case are nearly forty feet apart. An important use of the instrument will be in photographing the spectra of faint stars. These will be taken in two ways. A concave and convex lens are inserted near the focal plane of the telescope, and between this plane and the principal mirror. Their positions are such that between them the cone of rays of each star is parallel. A prism is inserted as described more fully in Harvard Circular 108. For measuring the approach and recession of faint stars, as described in Harvard Circular 110, a similar device is employed. As a large dispersion is required, the prism has such an angle that the cone of rays is inclined, and comes to a focus outside the tube of the telescope. The photographic plate, therefore, does not intercept any part of the incident rays. The reversed spectrum is formed by turning the lenses, prism, and plate 180°.

Cost.

To establish an observatory of the first class is a costly operation. The expenditure for plant, land, buildings, and instruments should be two or three hundred thousand dollars. The annual income of the Greenwich, Paris, Pulkowa, and Harvard observatories is about fifty thousand dollars, in each case. To secure this permanently at four per cent. the sum of $1,250,000 would be required. Accordingly, the total cost would be $1,500,000. To duplicate the resources of the U. S. Naval Observatory would involve an expenditure of at

least double this sum, or $3,000,000. A sixth part of the last named sum, or $500,000 would suffice to carry out the plan proposed in this paper. Figures can be given with a good deal of confidence since, at Harvard, we have had experience of a nearly similar character. A reflecting telescope of two feet aperture and its mounting have recently been constructed at a cost of less than $4,000. For a larger telescope we may assume that the cost of drawings and plans will be proportional to the first power, the cost of the machine- and hand-work to the square, and of the material, to the cube of the dimensions. At this rate, telescopes of six, seven, and eight feet aperture would cost $42,900, $63,000, and $87,400, respectively. The five-foot telescope at Harvard cost us much less than this rule would imply, but the conditions under which it was acquired were exceptional. Assuming the cost to be proportional to the cube of the dimensions, we have the cost in the three cases $108,000, $171,500, and $256,000. The actual cost would probably lie between these rather wide limits, but it is believed that a telescope of seven-feet aperture and mounting could be constructed for $150,000.

The current expenses can be closely estimated since, for seventeen years, the Harvard Observatory has conducted an auxiliary observing station in South America. Ten thousand dollars a year would be needed to carry on the proposed station satisfactorily. To produce this sum, allowing four per cent. interest, $250,000 would be required. With the income, three or four assistants could be maintained, who would keep the telescope at work throughout every clear night, and perhaps some smaller instruments. A certain amount of the income would be available for publication and for subsidies paid to astronomers here or in other countries, for assistants who would aid them in measuring and discussing the photographs. Before the large telescope is completed the interest on the principal would defray the expenses of the preliminary work of testing locations with smaller instruments, erecting houses for the observers and similar work. If the fund had an independent foundation, an additional $100,000 would be required for an executive for the management of the fund, etc. This would be saved if the superintendence would be undertaken without charge by the Harvard or some other existing observatory. The entire amount required would, there-

PROC. AMER. PHIL. SOC., XLV. 182D, PRINTED JUNE 25, 1906.

fore, be $400,000 or $500,000, which at most would only be a third of that required for an observatory of the first class and of the usual form.

Method of Administration.

The administration and management of the fund would, of course, rest with the donor. If it were left to me, I should at once write to the principal makers of glass for estimates of the cost and time required to furnish a disk of glass seven feet in diameter and one foot thick. An expedition to South Africa would next be planned, equipped with the two-foot reflector of the Harvard Observatory. This instrument would be mounted in the best available location, and regular work undertaken which would test the steadiness and other qualities of the atmosphere. Tests would also be made of various adjacent localities, with refracting telescopes of four, five, or six inches aperture. Meanwhile, correspondence would be opened with all those astronomers likely to give useful advice, and a committee would be formed of such astronomers as would attend a meeting at an early date. Thus, no time would be lost. The form of mounting would be the principal subject to be discussed at the first meeting, and the work of construction would be begun as soon as this point was settled. The results of the first expedition would probably serve to determine whether a better location could be found in South Africa than that we now occupy in Peru.

Discussion of Results.

Not only from its size, but from its exceptional location, this telescope ought to give better results than those previously obtained in almost every department of astronomical science. Its principal use will be in photography, determining the positions, brightness, and spectra of faint stars, especially novæ and variables, in depicting clusters and nebulæ, in studying the distribution of faint stars, in discovering and following faint satellites and asteroids, in measuring parallaxes and proper motions, and, in general, in studying all the properties of stars beyond the reach of smaller instruments.

In visual work, very high powers could be employed, without the difficulties usually encountered from diffraction when a very small emergent pencil is used. Owing, also, to the great light gathering

power, it is probable that visual observations of the surfaces of the outer planets, especially Uranus and Neptune, could be made to great advantage.

Evidently the material accumulated photographically would greatly exceed what could be properly discussed by a single individual or institution. Especial pains should be taken to place this material in the hands of any astronomers qualified to use it. The entire collection of photographs should eventually be kept together, where it could be consulted, but copies or enlargements of any portion or of the whole should be furnished at cost to any one desiring them. Qualified astronomers, ready to discuss any portion of the work, should be offered the use of the original negatives, given copies, and in every way aided in discussing and preparing the results for publication. It is desirable that they should be published in a separate series of quarto volumes. The fundamental principle should be that the results are for the world and not for a single individual, and every concession should be made to secure the widest use of the material collected. The telescope should be kept at work throughout every clear night. A scheme of work should be prepared every year by the aid of an international committee of astronomers, which should provide for a proper division of the time of the telescope, secure assistance and advice in discussing the results, and, in general, aid in obtaining the best administration. For instance, such a committee might spend several days together in New York, travelling and hotel expenses being paid, and care being taken that at least one European astronomer should be present each year. A German delegate might report that in his country a particular astronomer desired to study the distribution and brightness of the stars in globular clusters. A hundred hours might be assigned to this work and five photographs of each of ten clusters would be taken with exposures of two hours each. Contact prints would be made of these photographs and the originals sent to the German astronomer, who might be furnished with means for paying the salary of an assistant who would make the measures under his direction if the work was considered of sufficient importance. When the research was completed, the original negatives would be returned and added to the rest of the collection. The results would be printed

in the series of annals, giving the author as many copies as he could usefully distribute. On special occasions, as during an opposition of Mars, a specialist might be invited to the observatory and the telescope placed, for the time, at his disposal. It would be difficult to find useful work for the telescope when the moon was full. Such researches as photometric measures of the relative brightness of the components of close double stars and studies of the moon and planets could be made at such times.

Conclusions.

To sum up the results of this paper, it may be said that the desire to have the largest telescope in the world and to carry our knowledge of the stars farther than has ever been done before, has been very widespread. It would be unwise to construct a refracting telescope much larger than those already made. A million and a half dollars would be required to duplicate one of our present observatories of the first class. A reflecting telescope of seven feet aperture, larger and more powerful than any hitherto constructed, could be made at a moderate price. It should be mounted in the best possible location as regards climate, and preferably in the southern hemisphere, to permit the study of neglected regions. Such an instrument would produce, by photography, results in quality much better than can be obtained elsewhere and in such quantity that no single institution could discuss and publish them. These photographs should be distributed throughout the world, and astronomers of all countries would thus be furnished with better material for study than they could possibly obtain themselves. They would also be offered every aid in discussing and publishing their conclusions.

The estimated cost of carrying out this plan is not more than half a million dollars, or one third of that of an observatory of the usual form as now constructed. Not only would results be obtained superior to those now secured anywhere else, but the work would be planned, not by a single astronomer, but by an international committee of astronomers, and the results would be discussed by the most distinguished specialists in each department. In this way, following the example of the great industrial enterprises of the country, the plan of work would be improved continually in every detail.

It will be difficult during the twentieth century to make as great an advance in science as was done during the life of Franklin in the eighteenth century or after his death in the nineteenth century. How could the name of Franklin be more highly honored than by initiating this undertaking at his bi-centennial? Were he living, is there any way that would be more in accordance with his wishes and aims in life than to advance a science, not only in a direction, but by a method, which would bring together, as here proposed, experts from all parts of the world in a single field of work.

It is not easy for a man who by life-long work and skill has accumulated a large fortune to expend it wisely in science and to his own satisfaction. It is hard for him to see it wasted or yielding inadequate results. Money thus given should be expended, as it has been acquired, by careful management and the use of strict business methods, in order to obtain the greatest return for every disbursement.

The name of a donor could in no way be better immortalized than by associating it with such a real advance in the greatest problem to the solution of which the mind of man has aspired,—the study of the sidereal universe.

THE HUMAN HARVEST.

By DAVID STARR JORDAN.

(*Read April 18, 1906.*)

Science is wisdom set in order. It is known as science by its orderly arrangement, but above and beyond all matters of arrangement the wisdom itself must take rank. Wisdom is the essence of human experience, the contact of mind with the order of nature. Of all men of his time, Benjamin Franklin was preëminently a man of wisdom. By the same token the first leader in science in America, he still takes rank with the greatest.

So in this time of historic recognition, it is proper that a speaker of to-day should find his message in the words of Benjamin Franklin, and the message I choose is one for which this City of Philadelphia has always stood and from which it has taken its Greek name, the name which in classical phrase says with a single word that men are brothers worthy of our love. It is a message for which the State of Pennsylvania has always stood, for the same principle was embodied in the life of William Penn. This has always been a Quaker City, and the Quakers, the Friends, have been our best apostles of the gospel of "peace on earth, good will towards men," the culmination of social and political wisdom.

Benjamin Franklin once said, "All war is bad; some wars worse than others." Then, once again, in more explicit terms, referring to the dark shadow of war cast over scenes of peace, the evil of the standing army, Franklin said to Baynes:

"If one power singly were to reduce its standing army it would be instantly overrun by other nations. Yet I think there is one effect of a standing army which must in time be felt so as to bring about the abolition of the system. A standing army not only diminishes the population of a country, but even the size and breed of the human species. For an army is the flower of the nation. All

[1] Parton's "Life of Franklin," II, p. 572.

the most vigorous, stout, and well-made men in a kingdom are to be found in the army, and these men in general cannot marry."[1]

What is true of standing armies is far more true of armies that fight and fall; for as Franklin said again, "Wars are not paid for in war times: the bill comes later."

In the discussion of the principles involved in Franklin's words, I must lay before you four fragments of history, three stories told because they are true, and one parable not true, but told for the lesson it teaches. And this is the first: Once there was a man strong, wealthy and patient, who dreamed of a finer type of horse than had ever yet existed. This horse should be handsome, clean-limbed, intelligent, docile, strong and swift. These traits were to be not those of one horse alone, a number of a favored equine aristocracy, they were to be "bred in the bone" so that they would continue from generation to generation, the attributes of a special common type of horse. And with this dream ever before his waking eyes, he invoked for his aid, the four twin genii of organic life, the four by which all the magic of transformism of species has been accomplished either in nature or in art. And these forces once in his service, he left to their control all the plans included in his great ambition. These four genii or fates are not strangers to us, nor were they new to the human race. Being so great and so strong, they are invisible to all save those who seek them. Men who deal with them after the fashion of science give them commonplace names, variation, heredity, segregation, selection.

Because not all horses are alike, because in fact no two were ever quite the same, the first appeal was made to the genius of Variation. Looking over the world of horses, he found to his hand Kentucky race horses, clean-limbed, handsome and fleet, some more so and others less. So those which had the most of the virtues of the horse which was to be were chosen to be blended in new creation. Then again, he found thoroughbred horses of Arabian stock, hardy and strong and intelligent. These virtues were needed in the production of the perfect horse. And here came the need of the second genius, who is called Heredity. With the crossing of the racer with the thoroughbred, all qualities of both were blended

in the progeny. The next generation partook of all desirable traits and again of undesirable ones as well. Some the one, and some the other, for sire and dam alike had given the stamp of its own kind and for the most part in equal degree. But again never in a degree quite equal, and in some measure these matters varied with each sire and each dam, and with each colt of all their progeny. It was found that the progeny of the mare called Beautiful Bells excelled all others in retaining all that was good in fine horses, and in rejecting all that a noble horse should not have. And like virtues were attached to the sires called Palo Alto, Electricity and Electioneer.

But there were horses and horses; horses not of the chosen breed, and should these enter the fold with their common blood it would endanger all that had been already accomplished. For the ideal horse mating with the common horse controls at the best but half the traits of the progeny. If the strain were to be established, the vulgar horse flesh must be kept away, and only the best remain in association with the best. Thus Segregation, the third of the genii was called into service lest the successes of this herd be lost in the failure of some other.

Under the spell of Heredity all the horses partook of the charm of Beautiful Bells and of Electricity and of Palo Alto, for firmly and persistently all others were banished from their presence. There were some who were not strong, some who were not sleek, some who were not fleet, some who were not clean-limbed, nor docile, nor intelligent. At least, they were not so to the degree which the dream of fair horses demanded. By the force of Selection, all such were sent away. Variation was always at work making one colt unlike another; Heredity made each colt a blend or mosaic of traits of sire and of grandsires and granddams; Selection left only good traits to form this mosaic, and the grandsire and granddam, sire and dam, and the rest of the ancestry lived their lives again in the expanding circle of descent.

Thus in the final result, the horses who were left were the horses of their owner's dream. The future of the breed was fixed, and fixed at the beginning by the very framing of the conditions under

which it lived. It is variation which gives better as well as worse. It is heredity which saves all that has been attained—for better or for worse. It is selection by which better triumphs over worse, and it is segregation which protects the final result from falling again into the grasp of the general average. In all this, selection is the vital moving changing force. It throws the shaping of the future on the individual chosen by the present. The horse who is left marks the future of his kind. The history of the steed is an elongation of the history of those who are chosen for parentage. And with the best of the best chosen for parentage, the best of the best appears in the progeny. The horse-harvest is good in each generation. As the seed we sow, so shall we reap.

And this story is true, known to thousands of men. And it will be true again just as often as men may try to carry it into experiment. And it will be true not of horses alone, for the four fates which guide and guard life have no partiality for horses but work just as persistently for cattle or sheep, or plums or roses, or calla or cactus, as they do for horses or for men. From the very beginning of life they have wrought untiringly—and in your life and in mine—in the grass of the field, the trees of the forest—in bird and beast, everywhere we find the traces of their energy.

And this brings me to my second story, which is not true as history, but only in its way as parable.

There was once a man—strenuous no doubt, but not wise, for he did not give heed to the real nature of things and so he set himself to do by his own unaided hand the work which only the genii can accomplish. And this man possessed also a stud of horses. They were docile, clean-limbed, fleet, and strong and he would make them still more strong and swift. So he rode them swiftly with all his might—day and night, always on the course, always pushed to the utmost, leaving only the dull and sluggish to remain in the stalls. For it was his dream to fill these horses with the spirit of action, with the glory of swift motion, that this glory might be carried on and on to the last generation of horses. There were some who could not keep the pace, and to these and these alone he assigned the burden of bearing colts. And the feeble and the broken, the

dull of wit, the coarse of limb, became each year the mothers of the colts. The horses who were chosen for the race-course he trained with every care, and every stroke of discipline showed itself in the flashing eyes and straining muscles, such were the best horses. But the other horses were the horses who were left. From their loins came the next generation and with these then was less fire and less speed than the first horses possessed in such large measure. But still the rush went on—whip and spur made good the lack of native movement. The racers still pushed on the course, while in the stalls and paddocks at home, the dull and common horses bore their dull and common colts. Variation was still at work with these as patiently as ever. Heredity followed, repeating faithfully whatever was left to her. Segregation, always conservative, guarded her own, but could not make good the deficiencies. Selection, forced to act perversely, chose for the future the worst and not the best, as was her usual fashion. So the current of life ran steadily downward. The herd was degenerating because it was each year an inferior herd which bred. Each generation yielded weaker colts, rougher, duller, clumsier colts, and no amount of training or lash or whip or spur made any permanent difference for the better. The *horse-harvest* was bad. Thoroughbred and race-horse gave place to common beasts, for in the removal of the noble the ignoble always finds its opportunity. It is always the horse that remains which determines the future of the stud.

In like fashion from the man who is left flows the current of human history.

This tale then is a parable, a story of what never was. but which is always trying to become true.

Once there was a great king—and the nation over which he bore rule lay on the flanks of a mountain range, spreading across fair hills and valleys green and fertile across to the Mediterranean Sea. And the men of his race, fair and strong, self-reliant and self-confident, men of courage and men of action, were men "who knew no want they could not fill for themselves." They knew none on whom they looked down, and none to whom they regarded themselves inferior. And for all things which men could accomplish,

these plowmen of the Tiber and the Apennines felt themselves fully comptent and adequate. "Vir," they called themselves in their own tongue, and *virile,* virilis, men like them are called to this day. It was the weakling and the slave who was crowded to the wall; the man of courage begat descendents. In each generation and from generation to generation the human harvest was good. And the great wise king who ruled them; but here my story halts—for there was no king. There could be none. For it was written, men fit to be called men, men who are *Vires,* "are too self-willed, too independent, and too self-centred to be ruled by anybody but themselves." Kings are for weaklings, not for men. Men free-born control their own destinies. "The fault is not in our stars, but in ourselves that we are underlings." For it was later said of these same days: "there was a Brutus once, who would have brooked the Eternal Devil to take his seat in Rome, as easily as a king." And so there was no king to cherish and control these men his subjects. The spirit of freedom was the only ruler they knew, and this spirit being herself metaphoric called to her aid the four great genii which create and recreate nations. Variation was ever at work, while heredity held fast all that she developed. Segregation in her mountain fastnesses held the world away, and selection chose the best and for the best purposes, casting aside the weakly, and the slave, holding the man for the man's work, and ever the man's work was at home, building the cities, subduing the forests, draining the marshes, adjusting the customs and statutes, preparing for the new generations. So the men begat sons of men after their own fashion, and the men of strength and courage were ever dominant. The Spirit of Freedom was a wise master, cares wisely for all that he controls.

So in the early days, when Romans were men, when Rome was small, without glory, without riches, without colonies and without slaves, these were the days of Roman greatness.

Then the Spirit of Freedom little by little gave way to the Spirit of Domination. Conscious of power, men sought to exercise it, not on themselves but on one another. Little by little, this meant banding together, aggression, suppression, plunder, struggle, glory, and

all that goes with the pomp and circumstance of war. The individuality of men was lost in the aggrandizement of the few. Independence was swallowed up in ambition, patriotism came to have a new meaning. It was transferred from the hearth and home to the trail of the army.

It does not matter to us now what were the details of the subsequent history of Rome. We have now to consider only a single factor. In science, this factor is known as "reversal of selection." "Send forth the best ye breed!" That was the word of the Roman war-call. And the spirit of Domination took these words literally, and the best were sent forth. In the conquests of Rome, *Vir,* the real man, went forth to battle and to the work of foreign invasion, *Homo,* the human being, remained in the farm and the workshop and begat the new generations. Thus "Vir gave place to Homo." The sons of real men gave place to the sons of scullions, stable-boys, slaves, camp-followers, and the riff-raff of those the great victorious army does not want.

The fall of Rome was not due to luxury, effeminacy, corruption, the wickedness of Nero and Caligula, the weakness of the train of Constantine's worthless descendants. It was fixed at Philippi, when the spirit of domination was victorious over the spirit of freedom. It was fixed still earlier, in the rise of consuls and triumvirates and the fall of the simple sturdy self-sufficient race who would brook no arbitrary ruler. When the real men fell in war, or were left in far-away colonies, the life of Rome still went on. But it was a different type of Roman which continued it, and this new type repeated in Roman history its weakling parentage.

"It is puerile," says Charles Ferguson, "to suppose that kingdoms are made by kings. The kings could do nothing if the mob did not throw up its cap when the king rides by. The king is consented to by the mob, because of that in him which is mob-like. The mob loves glory and prizes, so does the king. If he loved beauty and justice, the mob would shout for him while the fine words were sounding in the air, but he could never celebrate a jubilee or establish a dynasty. When the crowd gets ready to demand justice and beauty, it becomes a democracy and has done with kings."

Thus we read in Roman history the rise of the mob and of the emperor who is the mob's exponent. It is not the presence of the emperor which makes imperialism. It is the absence of the people, the want of men. Babies in their day have been emperors. A wooden image would serve the same purpose. More than once it has served it. The decline of a people can have but one cause, the decline in the type from which it draws its sires. A herd of cattle can degenerate in no other way than this, and a race of men is under the same laws. By the rise in absolute power, as a sort of historical barometer, we may mark the decline in the breed of the people. We see this in the history of Rome. The conditional power of Julius Cæsar, resting on his own tremendous personality, showed that the days were past of Cincinnatus and of Junius Brutus. The power of Augustus showed the same. But the decline went on. It is written that "the little finger of Constantine was thicker than the loins of Augustus." The emperor in the time of Claudius and Caligula was not the strong man who held in check all lesser men and organizations. He was the creature of the mob, and the mob, intoxicated with its own work, worshipped him as divine. Doubtless the last emperor, Augustulus Romulus, before he was thrown into the scrap-heap of history, was regarded in the mob's eyes and his own as the most godlike of them all.

What have the historians to say of these matters? Very few have grasped the full significance of their own words, for very few have looked on men as organisms, and on nations as dependent on the specific character of the organisms destined for their reproduction.

So far as I know, Benjamin Franklin was the first to think of man thus as an inhabitant, a species in nature among other species and dependent on nature's forces as other animals and other inhabitants must be.

In Otto Seeck's great history of "The Downfall of the Ancient World" (Der Untergang der Antiken Welt), he finds this downfall due solely to the rooting out of the best ("Die Ausrottung der Besten"). The historian of the "Decline and Fall of the Roman Empire" or any other empire is engaged solely with the details of the process by which the best men are exterminated. Speaking of Greece, Dr. Seeck says, "A wealth of force of spirit went down in

the suicidal wars." "In Rome, Marius and Cinna slew the aristocrats by hundreds and thousands. Sulla destroyed the democrats, and not less thoroughly. Whatever of strong blood survived, fell as an offering to the proscription of the Triumvirate." "The Romans had less of spontaneous force to lose than the Greeks. Thus desolation came to them sooner. Whoever was bold enough to rise politically in Rome was almost without exception thrown to the ground. *Only cowards remained and from their brood came forward the new generations.* Cowardice showed itself in lack of originality and in slavish following of masters and traditions."

The Romans of the Republic could not have made the history of the Roman Empire. In their hands it would have been still a republic. Could they have held aloof from world-conquering schemes, Rome might have remained a republic, enduring even to our own day. The seeds of destruction lie not in the race nor in the form of government, but in the influences by which the best men are cut off from the work of parenthood.

"The Roman Empire," says Seeley, "perished for want of men." The dire scarcity of men is noted even by Julius Cæsar. And at the same time it is noted that there are men enough. Rome was filling up like an overflowing marsh. Men of a certain type were plenty, "people with guano in their composition," to use Emerson's striking phrase, but the self-reliant farmers, the hardy dwellers on the flanks of the Apennines, the Roman men of the early Roman days, these were fast going, and with the change in the breed came the change in Roman history.

"The mainspring of the Roman army for centuries had been the patient strength and courage, capacity for enduring hardships, instinctive submission to military discipline of the population that lined the Apennines."

With the Antonines came "a period of sterility and barrenness in human beings." *"The human harvest was bad."* Bounties were offered for marriage. Penalties were devised against race-suicide. "Marriage," says Metellus, "is a duty which, however painful, every citizen ought manfully to discharge." Wars were conducted in the face of a declining birth rate, and this decline in quality and quan-

tity of the human harvest engaged very early the attention of the wise men of Rome.

"The effect of the wars was that the ranks of the small farmers were decimated, while the number of slaves who did not serve in the army multiplied" (Bury).

Thus "*Vir* gave place to *Homo*," real men to mere human beings. There were always men enough such as they were. "A hencoop will be filled, whatever the (original) number of hens," said Benjamin Franklin. And thus the mob filled Rome. No wonder the mob-leader, the mob-hero rose in relative importance. No wonder "the little finger of Constantine was thicker than the loins of Augustus." No wonder that "if Tiberius chastised his subjects with whips, Valentinian chastised them with scorpions."

"Government having assumed godhead took at the same time the appurtenances of it. Officials multiplied. Subjects lost their rights. Abject fear paralyzed the people and those that ruled were intoxicated with insolence and cruelty." "The worst government is that which is most worshipped as divine." "The emperor possessed in the army an overwhelming force over which citizens had no influence, which was totally deaf to reason or eloquence, which had no patriotism because it had no country, which had no humanity because it had no domestic ties." "There runs through Roman literature a brigand's and barbarian's contempt for honest industry." "Roman civilization was not a creative kind, it was military, that is destructive." What was the end of it all? The nation bred real men no more. To cultivate the Roman fields "whole tribes were borrowed." The man of the quick eye and the strong arm, gave place to the slave, the scullion, the pariah, the man with the hoe, the man whose lot does not change because in him there lies no power to change it. "Slaves have wrongs, but freemen alone have rights." So at the end the Roman world yielded to the barbaric, because it was weaker in force. "The barbarians settled and peopled the barbaric rather than conquered it." And the process is recorded in history as the fall of Rome.

"Out of every hundred thousand strong men, eighty thousand were slain. Out of every hundred thousand weaklings, ninety to ninety-five thousand were left to survive." This is Dr. Seeck's cal-

culation, and the biological significance of such mathematics must be evident at once. Dr. Seeck speaks with scorn of the idea that Rome fell from the decay of old age, from the corruption of luxury, from neglect of military tactics or from the over-diffusion of culture.

"It is inconceivable that the mass of Romans suffered from over-culture." "In condemning the sinful luxury of wealthy Romans, we forget that the trade-lords of the fifteenth and sixteenth centuries were scarcely inferior in this regard to Lucullus and Apicius, their waste and luxury not constituting the slightest check to the advance of the nations to which these men belonged. The people who lived in luxury in Rome were scattered more thinly than in any modern state of Europe. The masses lived at all times more poorly and frugally because they could do nothing else. Can we conceive that a war force of untold millions of people is rendered effeminate by the luxury of a few hundreds?"

"Too long have historians looked on the rich and noble as marking the fate of the world. Half the Roman Empire was made up of rough barbarians untouched by Greek or Roman culture."

"Whatever the remote and ultimate cause may have been, the immediate cause to which the fall of the empire can be traced is a physical not a moral decay. In valor, discipline and science the Roman armies remained what they had always been and the peasant emperors of Illyricum were worthy successors of Cincinnatus and Caius Marius. But the problem was, how to replenish those armies. Men were wanting. The Empire perished for want of men" (Seeley).

Does history ever repeat itself? It always does if it is true history. If it does not we are dealing not with history but with mere succession of incidents. Like causes produce like effects, just as often as may may choose to test them. Whenever men use a nation for the test, poor seed yields a poor fruition. Where the weakling and the coward survives in human history, there "the human harvest is bad," and it can never be otherwise.

The finest Roman province, a leader in the Roman world, was her colony of Hispania. What of Spain in history? What of Spain to-day? "This is Castile," said a Spanish writer, "she makes men

and wastes them." "This sublime and terrible phrase," says another writer, "sums up Spanish history."

In 1630, according to Captain Calkins, the Augustinian friar, La Puente, thus summed up the fate of Spain:

"Against the credit for redeemed souls, I set the cost of armadas and the sacrifice of soldiers and friars sent to the Philippines. And this I count the chief loss: for mines give silver and forests give timber, but only Spain gives Spaniards, and she may give so many that she may be left desolate and constrained to bring up strangers' children instead of her own."

Another of the noblest of Roman provinces was Gallia, the favored land, in which the best of the Romans, the Franks and the Northmen have mingled their blood to produce a nation of men, hopefully leaders in the arts of peace, fatally leaders also in the arts of war.

To-day we are told by Frenchmen that France is a decadent nation. This is a confession of judgment, not an accusation of hostile rivals. It does not mean that the slums of Paris are destructive of human life. That we know elsewhere. Each great city has its great burdens, and these fall hard on those at the bottom of the layers of society. There is degradation in all great cities, but the great cities are not the whole of France. It is claimed that the decadence is deep-seated, not individual. It is said that the birth-rate is steadily falling, that the average stature of men is lower by two inches at least than it was a century ago, that the physical force is less among the peasants at their homes. Legoyt tells us that "it will take long periods of peace and plenty before France can recover the tall statures mowed down in the wars of the republic and the first empire." What is the cause of all this? Intemperance, vice, misdirected education, bureaucracy and the rush toward ready made careers? These may be symptoms. They are not causes. Demolins asks in that clever volume of his: "In what constitutes the superiority of the Anglo-Saxon?" Before we answer this, let us inquire in what constitutes the inferiority of the Latin races? If we admit this inferiority exists in any degree, and if we answer it in any degree, we find in the background the causes of the fall of Greece, the fall of Rome, the fall of Spain. We find the spirit of domina-

tion, the spirit of glory, the spirit of war, the final survival of subserviency, of cowardice and of sterility. The man who is left holds in his grasp the history of the future. The evolution of a race is always selective, never collective. Collective evolution among men or beasts, the movement upward or downward of the whole as a whole, irrespective of training or selection does not exist. As Lepouge has said, "It exists in rhetoric, not in truth nor in history."

The survival of the fittest in the struggle for existence is the primal moving cause of race progress and of race changes. In the red stress of human history, this natural process of selection is sometimes reversed. A reversal of selection is the beginning of degradation. It is degradation itself. Can we see the fall of Rome in the downfall of France? Let us look again at the history. A single short part of it will be enough. It will give us the clue to the rest.

In the Wiertz gallery in Brussels is a wonderful painting, dating from the time of Waterloo, called Napoleon in Hell. It represents the great marshal with folded arms and face unmoved descending slowly to the land of the shades. Before him, filling all the background of the picture with every expression of countenance are the men sent before him by the unbridled ambition of Napoleon. Three millions and seventy thousand there were in all—so history tells us, more than half of them Frenchmen. They are not all shown in one picture. They are only hinted at. And behind the millions shown or hinted at are the millions on millions of men who might have been and are not—the huge widening human wedge of the possible descendants of the men who fell in battle. These men of Napoleon's armies were the youth without blemish, "the best that the nation could bring," chosen as "food for powder," "ere evening to be trampled like the grass," in the rush of Napoleon's great battles. These men came from the plow, from the work-shop, from the school, the best there were—those from eighteen to thirty-five years of age at first, but afterwards the older and the younger. "A boy will stop a bullet as well as a man"; this maxim is accredited to Napoleon. "The more vigorous and well born a young man is," says Novicow, "the more normally constituted, the greater his chance to be slain by musket or magazine, the rifled cannon and other similar engines of civilization." Among those destroyed by Napoleon were "the élite

of Europe." "Napoleon," says Otto Seeck, "in a series of years seized all the youth of high stature and left them scattered over many battle fields, so that the French people who followed them are mostly men of smaller stature. More than once in France since Napoleon's time has the military limit been lowered."

I need not tell again the story of Napoleon's campaigns. It began with the United States, the justice and helpfulness of the Code Napoléon, the prowess of the brave lieutenant whose military skill and intrepidity had caused him to deserve well of his nation.

The spirit of freedom gave way to the spirit of domination. The path of glory is one which descends easily. Campaign followed campaign, against enemies, against neutrals, against friends. The trail of glory crossed the Alps to Italy and to Egypt, crossed Switzerland to Austria, crossed Germany to Russia. Conscription followed victory and victory and conscription debased the human species. *"The human harvest was bad."* The first consul became the emperor. The servant of the people became the founder of the dynasty. Again conscription after conscription. "Let them die with arms in their hands. Their death is glorious, and it will be avenged. You can always fill the places of soldiers." These were Napoleon's words when Dupont surrendered his army in Spain to save the lives of a doomed battalion.

More conscription. After the battle of Wagram, we are told, the French began to feel their weakness, the Grand Army was not the army which fought at Ulm and Jena. "Raw conscripts raised before their time and hurriedly drafted into the line had impaired its steadiness."

On to Moscow,[1] "amidst ever-deepening misery they struggled on, until of the 600,000 men who had proudly crossed the Niemen for the conquest of Russia, only 20,000 famished, frost-bitten, unarmed spectres staggered across the bridge of Korno in the middle of December."

"Despite the loss of the most splendid army marshalled by man, Napoleon abated no whit of his resolve to dominate Germany and discipline Russia. "... He strained every effort to call the youth

[1] These quotations are from the "History of Napoleon," I, by J. H. Rose.

of the empire to arms . . . and 350,000 conscripts were promised by the Senate. The mighty swirl of the Moscow campaign sucked in 150,000 lads of under twenty years of age into the devouring vortex." "The peasantry gave up their sons as food for cannon." But "many were appalled at the frightful drain on the nation's strength." "In less than half a year after the loss of half a million men a new army nearly as numerous was marshalled under the imperial eagles. But the majority were young, untrained troops, and it was remarked that the conscripts born in the year of Terror had not the stamina of the earlier levies. Brave they were, superbly brave, and the emperor sought by every means to breathe into them his indomitable spirit." "Truly the emperor could make boys heroes, but he could never repair the losses of 1812." "Soldiers were wanting, youths were dragged forth." The human harvest was at its very worst.

And the sequel of it all is the decadence of France. In the presence of war—of war on such a mighty ruthless and ruinous scale—one does not have to look far to find in what constitutes the superiority of the Anglo-Saxon. And we see the truth in Franklin's words, the deeper truth of their deeper wisdom: "Men do not pay for war in war time; the bill comes later."

Another wise man, Ralph Waldo Emerson, has used these words: "Man has but one future, and that is predetermined in his lobes." "All the privilege and all the legislation in the world cannot meddle or help. How shall a man escape from his ancestors or draw off from his veins the black drop?"

It is related that Guizot once asked this question of James Russell Lowell, "How long will the republic endure?" "So long as the ideas of its founders remain dominant," was the answer. But again we have this question: "How long will the ideas of its founders remain dominant?" Just so long as the blood of the founders remains dominant in the blood of its people. Not necessarily the blood of the Puritans and the Virginians alone, the original creators of the land of free states. We must not read our history so narrowly as that. It is the blood of free-born men, be they Roman, Frank, Saxon, Norman, Dane, Goth or Samurai. It is a free stock which creates a free nation. Our republic shall endure so long as the

human harvest is good, so long as the movement of history, the progress of peace and industry leaves for the future not the worst but the best of each generation. The Republic of Rome lasted so long as there were Romans, the Republic of America will last so long as its people, in blood and in spirit, remain what we have learned to call Americans.

By the law of probabilities as developed by Quetelet, there will appear in each generation the same number of potential poets, artists, investigators, patriots, athletes and superior men of each degree.

But this law involves the theory of continuity of paternity, that in each generation a percentage practically equal of men of superior force or superior mentality should survive to take the responsibilities of parenthood. Otherwise Quetelet's law becomes subject to the operation of another law, the operation of reversed selection, or the biological "law of diminishing returns." In other words, breeding from an inferior stock is the sole agency in race degeneration, as selection natural or artificial along one line or another is the sole agency in race progress.

And all laws of probabilities and of averages are subject to a still higher law, the primal law of biology, which no cross-current of life can overrule or modify: *Like the seed is the harvest.*

HEREDITY AND VARIATION; LOGICAL AND BIOLOGICAL.

By WILLIAM KEITH BROOKS.

(*Read April 20, 1906.*)

One need know little of the current literature of biology to be aware that many hypotheses have been proposed to account for the resemblance of offspring to parent. This resemblance is commonly held to be due to the transmission of a substance of inheritance, and we are told that this substance is the residence of the species and the bearer of its qualities.

Reproduction is the transmission of living matter of some sort, and it is part of the legitimate work of biology to discover, by the scientific method of observation and experiment, what it is in the transmission of which reproduction consists; but it by no means follows that there is meaning in our words when we call that which is thus transmitted in reproduction the substance of heredity, or the bearer of the species.

So far as the word is used inductively in biology, heredity is the resemblance of child to parent, of offspring to ancestor, while the difference between child and parent is called variation. These words are also used metaphorically to designate the cause or the explanation of the resemblances and differences between descendants and ancestors, just as gravitation is used metaphorically to designate that which makes things gravitate, geotropism that which makes roots grow downwards, and selection that which brings about survival in the struggle for existence. In what I have to say I shall restrict myself to the inductive meaning of the words, for I know that your thoughts are so free from the bonds of metaphysics that you know we accomplish nothing by saying that heredity makes beings inherit, or that variation makes them vary, or that selection selects.

Let us consider the word inheritance as a term to designate the resemblance between child and parent. You know that while the descendant does, on the average, resemble its ancestors and collateral

relatives more than it resembles anything else in nature, it is never identical with them. We say, in our careless way, that organisms exhibit specific identity behind or in spite of their individual diversity, when all we mean is that, while they resemble their parents, they are never identical with them. This diversity in unity is common to all natural objects, but it is most impressive in familiar living beings, in our friends and acquaintances, in our dogs and horses, and in the plants that we tend with our own hands. We may think of the casual stranger in the crowded street, or the unknown citizen of Timbuctoo, or the stalks in the cornfield that we pass in the train, as representatives of species and nothing more, but all the living beings we know practically we know as individual members of their kind.

If we are permitted to reason from the living beings we know best to those that concern us less, we must conclude that every living being is a unique member of its kind. It is more like its kind than like anything else in nature, but it is unique for there is nothing else in nature just like it. Reproduction is not the generation of like by like in any literal or mathematical sense. It is, rather, the generation of unique beings that are, on the average, more like their allies than they are like anything else in nature. We may for our own purposes, and in our minds, consider their kinship apart from their individuality, but this does not show that their kinship is separated from their individuality in fact. Living beings do not exhibit unity and diversity, but unity in diversity. These are not two facts but one. The delight of intimate acquaintance with animals is due to the inseparableness of their specific unity from their individuality, and our attempts to separate in our minds what is not separable in fact lead us to two narrow and imperfect views of the facts, two crude and unfinished mental concepts, neither of which corresponds to anything in nature.

All this is familiar, but I ask you to reflect upon it, to decide for yourselves whether it does not mean that inheritance or resemblance to ancestors, and variation or difference from ancestors, are only imperfect mental concepts; crude ideas, and not facts; whether the fact is not the individuality in kinship of living beings. Each of you must answer this simple question for himself. I cannot regard them

as facts, as they seem to me to be only imperfect ideas of facts, mental states which have arisen through a partial and uncritical view of our experience, to the neglect of that which has not interested us nor seemed to concern us.

If you agree with me that resemblance to ancestors does not exist in nature separated from individuality or difference from ancestors, that inheritance is not a fact but an imperfect idea of facts, admitting of improvement by comparison with nature, and in no other way,—if you agree to this, what becomes of the notion of a substance of inheritance? There is, no doubt, a material equivalent for every mental concept, and the material equivalent of heredity may be in the brain of the speculative philosopher, for I cannot find it in living beings nor in germ cells nor in chromatin.

I hope you will not accuse me of opposing the scientific study of inheritance and variation, for nothing is farther from my intention. The resemblances and differences between ancestors and descendants are as worthy of study as arithmetic, which has been of inestimable value to mankind although there is in nature no quantity without quality.

We cannot make progress in natural knowledge without specializing; picking out what interests us and ignoring what does not seem to concern us; but specialization is not an unmixed benefit, and if it blinds our eyes to the real world that lies before them it may prove to be an unmitigated evil; leading the modern scientific man into the forlorn agnosticism of the ancient philosophers who held that we can never know anything because no real thing exists abstractly. Things do not cease to be because we fail to note them, and when we fix our attention upon some partial and imperfect conception of nature to the neglect of that which does not interest us, we may forget the reality of that which we have failed to consider, and we may thus be led to opinions which seem to be the logical conclusions of sound reasoning when they are but new illustrations of the threadbare fallacy of the undistributed middle—the fallacy which comes from mistaking a part for a whole.

The paradoxes into which the biologists fall in their efforts to locate the substance of inheritance remind me of the perplexity of the school boy, who, having tried to add together six horses and

nine cows and five apples, wonders whether the result is horses or cows or apples. If he were to attribute the virtue of his arithmetic to a substance of numeration and to wonder whether it resides in cows or apples, he would be still more like those who speculate about the location of the substance of inheritance, and think they have put their finger on it when they have called it idioplasm.

If you choose to declare that my contention, that inheritance is not a fact, is a metaphysical subtilty, I cannot help it. Call me a metaphysician if you will. But may it not be the speculative biologist who hunts in germ cells and in their chromatin for the physical basis of the crudity of his ideas who is the true metaphysician, and not I, who plead for nothing but the correction of our scientific concepts and their reduction to exactness by comparison with nature?

Science is making marvellous revelations of the order that pervades the apparent disorder of nature, showing us, by the method of analysis and comprehension, the most wonderful and admirable evidence of regularity in the course of events that had seemed to be chaotic, but this statistical method deals with averages while the natural world is concrete. No living being is a statistical average, and it is the peculiar task of biological science to recall our attention to the diversity of the statistical data, thus making equally marvellous and equally instructive revelations of the inexhaustible variety and boundless wealth of nature, for science deals with progress and discovery, not with finality, and the test of truth is nature and not logic.

Statistical science shows that there is, on the average, about one chance in some thousands that the average human being will commit murder or suicide within the year, but my friend is not a two hundred thousandth of a murderer, and I prize him because there is no one like him.

The biometrician tells us of a standard or norm, from which living beings recede by variation, and to which they approximate by heredity, but the normal or average living being does not exist in nature. The student of statistical science talks glibly of the normal man as if he were a public character, the familiar acquaintance of men of intellect, and a well known face to even the common herd. The biologist declares that he knows no such person;

that all men are particular men, concrete and unique; that the normal man is a fictitious character, a statistical average, reached by ignoring all that is distinctive of each human being.

One can easily see why the notion that species is in germ cells has come to prevail. Nothing in nature, except the human mind, is easier to contemplate as an independent, self-sustaining, self-sufficient whole than is an egg. The symbolical comparison of the universe to an egg appeals to all, for nothing is easier than to think of an egg as a metaphysical thing in itself, a self-centred and self-sufficient microcosm. For many of the practical purposes of the scientific embryologist it is convenient and legitimate to regard it as a compete and self-sufficient being, but one must not forget what these practical purposes are, for the use of a concept for a practical purpose is apt to end in belief that it is true in general and useful for all purposes, and thus to entangle one in unforeseen paradoxes.

Every reflective biologist must know that no living being is self-sufficient, or would be what it is, or be at all, if it were not part of the natural world, although no truth is easier to lose sight of. Living things are real things, and we can never know too much about them, but their reality is in their interrelations with the rest of nature, and not in themselves.

Surely, this is good sense and good science. No physiologist who studies the waste and repair of living bodies; no naturalist who knows living beings in their homes; no experimental embryologist who studies the influence of conditions, internal and external, upon development, should, for an instant, admit that a living being is self-sustaining or self-sufficient, or that its being is in itself; for the line we draw, for our own convenience, between living things and the external world, is not one that we find in nature, but one that we make for our own purposes.

The external world of a living being is as essential to it as its histological structure. If the environment of its body, or of any cell within its body had been different, neither cell nor body would be what it is, and if they had no environment they would not be at all, for neither seeds nor eggs nor desiccated rotifers exist ab-

stractly. A self-sufficient and self-sustaining living organism, whose being is in itself, is as fabulous as a griffin or a centaur, but no naturalist thinks, for an instant, that this truth casts any doubt upon the real existence of living things. While living things are real their reality or being is not absolute but dependent and relative.

One modern school of embryologists tells us that while the development of the egg into an individual organism is due to the reciprocal interaction between the germ and its environment, the species is in the germ as it is in itself; because, if it were not, like could not produce like. Like never does produce like, in any literal or absolute sense. If what has come about once may come about again under like conditions; it is among the possibilities of nature that a new animal kingdom, as rich and diversified as the one we know, might arise, in course of ages, from a starting point in the germ cells of some modern animal; for we know of nothing in the architecture of germ plasm that forbids.

If I venture at this late day to point out that ancestral development may be as epigenetic, from beginning to end, as individual development, and that the species for which we are seeking is not, and cannot be in the germ, I do so because the discovery is neither new nor original with me. It is so old that " up to date " zoologists tell us it is antiquated, abandoned, no longer worthy the attention of advanced thinkers.

According to this view, the species is not in chromatin, nor in germ cells, nor in differentiated cells, nor in gemmules, nor in idioplasm, nor in biophores nor in allelomorphs, nor in living beings at any stage of their existence, nor in the conditions of existence, because it is in that reciprocal interaction between the living being and the natural world, of which it is a part, which has been called the struggle for existence. Neither the stability of species nor the mutability of species is in living beings, because it is through extermination in the struggle for existence that the type is kept true to its kind, and also through this struggle that it becomes slowly changed.

You will note that it is as great an error to locate species in the external world as it is to locate it in germ cells, or in chromatin. It

neither exists in the organism nor in the environment, because it is in the reciprocal interaction between the two. The biological types of which the biometricians tell us are neither external standards to which living beings approach and from which they recede by variation, nor are they standards fixed in living beings by heredity. Inheritance and variation are not two things, but two imperfect views of a single process, for the difference between them is neither in living beings nor in any external standard of extermination, but in the reciprocal interaction between each living being and its competitors and enemies and sources of food and the others conditions of life.

If the being of the individual organism is not in itself, but in the reciprocal interaction between it and its environment, and if the being of species is not in germ cells but in the reciprocal interaction between living beings and their environment, then the being of the canine species is of the same sort as the being of a dog, and that of everything else in nature.

Is it as a self-sufficient thing in itself, or as part of the universe, that the stone exhibits gravitation? "When Sir Isaac Newton made his speech about the child and the pebble: "Did he mean," asks Dr. Holmes, "to speak slightingly of a pebble? A body which knows all the currents of force that traverse the globe; which holds fast by invisible threads to the ring of Saturn and the belt of Orion." "This is certain," says Locke, "things however absolute and entire they seem in themselves, are but retainers to other parts of nature, for that which they are most taken notice of by us. Their observable qualities, actions, and powers, are owing to something without them; and there is not so complete and perfect a part that we know of nature, which does not owe the being it has, and the excellencies of it, to its neighbours; and we must not confine our thoughts within the surface of any body, but look a great deal farther, to comprehend perfectly those qualities that are in it."

Since these things are true, is it not time to have done, once for all, with the metaphysical, pre-Darwinian notion of species, as something that resides in germ cells and is handed down by a substance of heredity?

THE ELIMINATION OF VELOCITY EFFECTS IN MEASURING PRESSURES IN A FLUID STREAM.

By FRANCIS E. NIPHER.

(*Read April 20, 1906.*)

In determining either velocities or pressures in a current of air or water within a pipe, it has always been found exceedingly difficult to obtain a value which has any physical meaning.

A straight tube thrust through the side of the pipe, terminating at some point within, and connected to a guage, will transmit the pressure, but on account of the draught of the fluid across the open end of the tube, the fluid will also be drawn out, by an atomizer action.

If on the other hand the tube is in the form of a Pitot tube with its mouth directed towards the advancing current, it transmits both static and dynamic pressure to the gauge.

In the case of a building or other structure in a stream of air, if it is desired to find the variation of pressure on the surface due to the wind, any form of barometer gives misleading results by reason of the compressions and rarefactions around the instrument itself. Moreover the wind sweeps across or into various openings leading to the mercury surface or flexible diaphragm. This introduces disturbances or errors, which cannot be corrected. Engineers are accustomed to combine with the Pitot tube, a similar tube with its opening at right angles to the stream lines in a pipe, but they usually give warning that the atomizer action of the air blowing across the opening is likely to give misleading results, if the velocity is great.

In order to study the Pitot tube in a manner that would eliminate statical pressure, a long series of experiments was made from the window of a car. The tube was so mounted that the mouth of the tube could be directed at any angle, the position being determined

on a graduated circle. The mouth of the tube could thus be directed towards the head of the train, towards the sky, the rear of the train, towards the ground and then towards the head of the train again. The setting could be made for each ten degrees. The zero angle was taken when the mouth of the tube was directed towards the head of the train, the pressure being then a maximum. The tube had a diameter of one inch, and the walls of the tube were of thin metal. For angles between 0 and 60°, the pressures were positive. At 60°, the pressure observed was zero. At an angle of 90°, the decrease in pressure was greater than the increase when the angle was zero.

The values observed are here given, the pressures being in grams per square centimeter. These values may, of course, be represented by an harmonic series. It requires about ten terms.

α	P	α	P
0	1.34		
10	1.30	100	—1.29
20	1.26	110	—1.03
30	1.21	120	—0.93
40	1.03	130	—0.89
50	0.63	140	—0.86
60	0.00	150	—0.86
70	—0.63	160	—0.79
80	—1.10	170	—0.66
90	—1.44	180	—0.54

These values are plotted in a polar diagram in Fig. 1.

The pressures were measured with a water manometer, having its tube inclined to a slope of one in twenty. The actual readings were, therefore, twenty times the numbers above given.

These values are the means of twenty measurements. The average velocity of the train as determined by simultaneous measurements with another Pitot tube was 41.8 miles per hour. The relative velocity of the air with respect to the train at the point where the measurements were made was 32.5 miles per hour. This lesser relative velocity was due to the fact that air was dragged along with the train.

It is therefore evident that in such a stream of air, a tube which is to eliminate the velocity effects, must be set with its mouth

directed at an angle of 60° with the stream lines, and not at an angle of 90° as is the usual custom.

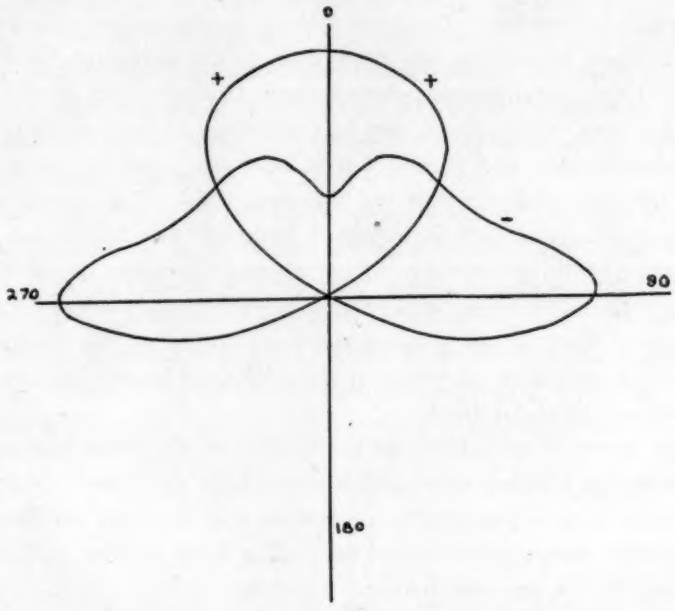

FIG. 1.

Two tubes rigidly connected and having their axes at an angle of 60° inserted into a pipe through which air is flowing will separate velocity pressure from staticial pressures. One is to be so turned in the stream that the reading of its gauge is a maximum. It will indicate both pressures. The other indicates only the actual pressure due to resistance of the pipe in advance of that point, if the discharge is into the air. Such statical pressure differences at two points along the pipe, will correspond to voltmeter readings between the points on a wire carrying an electric current.

Another means of eliminating velocity effects is given by a device which I here present.

A tube to carry the pressure to the gauge, terminates at the point where the pressure is to be collected, in a thin circular disk, placed edge-wise in the stream. A parallel disk is secured to

it by soldered or brazed rivets, with four or five circular sheets of wire cloth loosely clamped between the disks. The bundle of wire cloth sheets projects to some distance beyond the metal disks. On either side of the disk-collector so formed, a couple of sheets of wire cloth are laid, thus hiding the metal disks in the wire-cloth bundle.

With this arrangement, the pressure at the mouth of the collecting tube is wholly unaffected by the motion of the current of air. The compression and rarefaction around the tube are prevented from affecting this pressure by the two disks. The compression and rarefactions around the edges of the wire cloth bundle are also eliminated by the projecting margin around the edges of the disks, so that these compression and rarefaction effects do not get between the disks. They do get between the disks and affect the reading in a way that cannot be corrected, if the wire cloth layer does not project beyond the metal disks.

The layers of wire cloth on the outside of the disks is a recent improvement, which is necessary for very high velocities. It serves to smooth away irregularities in outline and prevents rarefactions due to these irregularities in the projecting layer of wire cloth from affecting the air pressure between the disks.[1]

When this disk collector stands edge-wise in free air in a current of air from a ten inch pipe delivering 96 cubic feet per second, the gauge connected with it is not in the least affected.

For comparatively small velocities and uniform pressures, as in pipes connected with blowers, the tube piercing one disk of the collector may be very small for a distance of an inch, and then widen in order to give necessary stiffness. The wire cloth layer need not then be over an inch in diameter, and the metal disks need not be over half an inch in diameter.

In measuring wind pressures on buildings where the pressures vary rapidly the tube should be a quarter of an inch in diameter, and the disks should be three and one-half inches and the wire cloth layers from five to five and one-half inches in diameter. The disks are placed flatwise near the wall.

[1] *Trans. Acad. of Sc. of St. Louis,* VIII.: 1.

These disk collectors may also be used with perfect results in water pipes, if the velocity is not great enough to produce discontinuities where air would show rarefactions. No tests have yet been made under these conditions. For high velocities the action of the Pitot tube deserves a complete re-investigation. It seems probable that the tube inclined at an angle of 60° with a Pitot tube will be the best method of eliminating velocity effects in a current of water at very high velocity.

In measuring wind pressures on buildings by means of inclined water gauges, the open end of the tube must be connected with a large reservoir, which is connected to a horizontal disk collector, far above the building. The disk is to be horizontal, so that the air current strikes it edgewise. These disks are now being mounted on the physics building of Washington University. Nearly a mile of piping is being used in connecting the various collectors, each

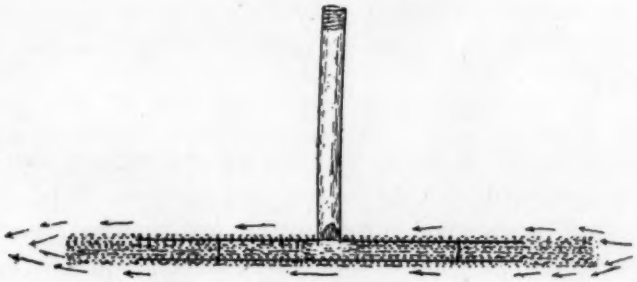

FIG. 2.

with its own gauge. The details of the collector are shown in Fig. 2, in cross section.

The two collectors here shown are presented to the society.

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THE PRESENT STATE OF THE QUESTION REGARDING THE FIRST PRINCIPLES OF THEORETICAL SCIENCE.

By JOSIAH ROYCE.

(*Read April 19, 1906.*)

I venture to use this opportunity to call attention to the existence and to the spirit of certain researches in which a good many well-equipped students of science are now taking part, and in which I myself, although very ill prepared for the work, have already tried in a very modest way to take some part myself. These researches interest primarily logicians, and to some extent mathematicians. They have a relation, however, not only to general philosophy, but also to the interests of a good many students of the special sciences. Let me try briefly to indicate the problems which give rise to such researches.

I.

Science, as we all know, has two aspects, namely, that aspect which is concerned with discovering and reporting facts, and that aspect which is concerned in constructing and applying theories. A scientific theory is a body of assertions connected together by processes of logical reasoning, and so chosen as to be of use in displaying the rational connections of facts, and in predicting facts which have not yet been observed. The extent to which theories are of use for the work of a given science varies very greatly with the stage of evolution which the science has reached, with the character of the subject matter and with the interests which control our study of the facts in question. Celestial mechanics furnishes an instance of a very highly developed theoretical branch of science. The extent to which theory is significant and successful in any one science, as for instance in biology or in chemistry, is in case of each such branch of scientific inquiry a kind of test of the stage which the science in question has reached in its evolution. In the history of

a science the premature prominence of theoretical constructions leads to a neglect of facts or to a too easy contentment with an insufficient collection of facts. When a science, however, is already highly developed but is also rapidly growing, the search for new facts is commonly guided by more or less highly developed theoretical interests, and is directel by presuppositions, by hypotheses, by questions, which have come to mind in consequence of reasonings due to theories.

In the normal case of a science in which theories play an important part, a scientific theory takes the form, first, of the statement of a set of principles, or of relatively fundamental propositions, which the theory treats, at least provisionally, as true. Secondly, the theory consists of the logical development of a set of consequences, which follow from these principles and so will be true in case the latter are true. These consequences may be reached, and in the case of the most highly developed theories, are reached, by mathematical computations. In its application to the work of the science, the theory becomes useful, in so far as its results can be compared with the particular facts of experience, or can be tested by seeing how far they lead to successful predictions.

A theory whose results disagree with facts has to be amended accordingly, but in many cases may be adjusted to the facts by alterations which leave its main principles intact, and which involve only minor modifications. When a theory succeeds up to a certain point, but leaves some facts indeterminate, it frequently gives rise to hypotheses concerning phenomena as yet unobserved, and in this sense may prove a guide to investigation. There are well known and important cases where a theoretical computation disagrees for a time with actually observed facts, but where the discrepancy can be shown to be due to the non-recognition of certain facts which, so soon as you take them into account, enable the original theory to apply with reasonable accuracy to the whole system of facts in question. In some cases improved methods of calculation, or other purely logical developments of theory itself, suffice to remove discrepancies; and such cases furnish very persuasive tests of the value of the theory in question.

If you look towards the world of facts, as experience shows them

to you, the principal use of a theory seems to lie in two things. First, a theory, if successful, enables you to give an economical description of a vast number of facts. Secondly, a theory usefully guides your search after new facts, and in particular your predictions, and the practical activities by means of which you apply your science to the study of new cases. The common mind often opposes theory and practice. But every enlightened student is aware how large a part theory plays, in those cases where theory is possible, as a means towards guiding the practical applications of a science to the various arts. Thus without astronomical theory the application of astronomy to navigation would remain very limited; because one who has to apply observations of the heavenly bodies to the work of the navigator must accomplish his application by means of definite processes of computation. Such computations can be reduced to precise rules only by means of considerations which belong to the theoretical side of the science. So long as, for the Babylonian astrologers, astronomy remained a mysterious branch of empirical natural history, computations could have only a limited scope. It is astronomical theory, not to be sure the whole of astronomical theory, but a certain limited portion of it, which gives to the navigator's computations a uniform and controlable character. Economical description, controlable application to the search for new facts, and to the practical uses of a science, these are the characters which a scientific theory must possess in order to meet the requirements which the real world makes upon it.

But these requirements cannot be met unless the theory possesses a certain coherent logical structure. This structure might in general be possessed, almost or quite equally, by a great number of different theories whereof only one happened to be true to the facts. Nevertheless, although the internal logical finish of the structure of a theory is by itself no guarantee that the theory is useful in describing or predicting facts, such logical structure is a condition sine qua non of a good theory. A natural question arises as to what constitutes this internal logically coherent structure to which a highly developed theory must conform. The question so stated may appear at first sight very vague and indeterminate. A theory, you will say, must make use of principles which it provisionally assumes to be

true. It must then develop the consequences of these principles. It must be logically accurate, and as full in its development of consequences as the application which is to be made of the theory seems to require. This, it would seem, can in general be said. But with this vague generality the theory of what constitutes a good theory would appear at first sight to be completed.

Yet a moment's thought will show, that we all pretend to know more about the structure towards which highly developed theories tend, than this first generalization would make manifest. For instance, it is a comment which has become commonplace, that, wherever quantitative conceptions are possible, theories whose first principles can be expressed in quantitative form, have a formal advantage over theories which have to be expressed in non-quantitative terms. Some portions of our empirical world are subject to measurement. Measurement in practice gives results which vary within the limits of error, and which are therefore inexact. A theory which is to be just to any highly advanced state of knowledge regarding measurable facts, must make use of principles which involve provisionally assumed relations of quantities. One advantage which a quantitative theory can then possess lies in the very fact that its provisionally assumed principles may be stated with an exactness which empirical measurements never reach. In other words, the very incapacity of our theory to account for the variations, and for the inexactness of any single process of measurement, may be an advantage in the development and in the further application of the theory. Assuming exact relations, and invariant relations, where the actual measurements of observers show a considerable range of uncontrollable variation, the theory may enable computations to be made, in terms of which the work of measurement may be guided, and the essential and unessential elements of experience may be distinguished. Cases of this sort suggest that the structure of theories is subject to certain logically definable laws which are somewhat independent of the precise degree to which in a given case the theory in question can be verified. In other words, while the true theory, in the sense of the theory that agrees with the observed facts, is indeed the ideal, one may be able to judge the value of a theory in advance of knowing whether it is true of not, in so far, for instance, as a quantitative

theory is preferable to a non-quantitative one, and in so far as exact theoretical interpretations are preferable to inexact ones.

This very commonplace instance suggests where lies the logical problem regarding the internal structure of theories. What does one mean, for instance, by a quantitative theory? In order to answer this question one must know what one means by quantity. Why are quantitative ideas more useful than non-quantitative ideas? Wherein lies the logical difference between conceptions of quantity and other conceptions? Is the notion that quantitative conceptions stand alone amongst possible scientific conceptions, in their peculiar possession of exactness, and of a capacity to be submitted to precise and entensive processes of deduction, is this presupposition itself well founded? Are there other concepts which are logically as exact as the quantitative concepts, which are as capable of being subjected to elaborate processes of a deductive character? If so, are there other regions than those of the sciences of measurement in which highly developed theoretical finish is possible? May the science of the future come to use other than quantitative theories in dealing with regions of nature or of mind where measurement proves to be unattainable, or inexact? How will the non-quantitative theories, in so far as they can be developed, stand related to the quantitative theories? What is it that makes certain concepts adapted to furnish a wide range of unexpected results, which can be reached deductively, and by exact devices of thinking, although these results cannot readily be seen at a glance, by merely inspecting the conceptions in question? How can mere deduction lead to an infinite number of unexpected results, as is often the case in the exact sciences? Do the possible conceptions which the human mind can frame, and can lay at the basis of theoretical constructions, form anything like a closed system? In other words, is the range over which our theoretical constructions vary simply limitless, and indeterminate, or is it, even if infinite, still in some way itself determinate, so that one can name certain fundamental concepts which every theory must use, or from which every theoretical construction must make a selection, even in defining its provisionally assumed principles? In other words, are there first principles of scientific theory? Are the ideas which we can use in defining our provisional hypotheses, in

initiating processes of logical deduction, ideas of which some general and thoroughgoing account is possible, so that, although we cannot predict the facts of the natural world, we can predict the forms in terms of which we shall always be obliged to think the rational connections of these facts in case we form any theory at all? Are, then, the internal conditions of theoretical science, the logical possibilities upon which such a science depends, of a determinate range, and of a knowable character? Such are the problems which are suggested when we begin to inquire as to the logical position which quantitative theories hold amongst the various types of theories which are logically possible.

The questions thus suggested are obviously of the most fundamental importance for any one who is interested in understanding the workings of science. Science depends upon finding facts; it certainly also aims at the controlling of facts. The control which is here in question may either mean the technical mastery of facts, the power to produce them at will, or it may mean the prediction of facts. But either kind of control is possible only in so far as we possess something of the nature of a theory. And a theory involves the construction and control, and logical linking of concepts which have to be of our own making. Therefore, the study of the types of concepts which we can construct and control and link, the study of the forms and linkages which the nature of our thought makes possible, is surely as serious a study, as the direct study of the facts which we can hope to control through the use of our intelligence. The pursuit of useful knowledge surely includes in the end a knowledge of those logical processes of thought whereby we come to make an intelligent use of facts.

II.

The result of such considerations is that a science is needed which I may provisionally call the morphology of theories. This science is a branch of logic. And it is to this science that I now call your attention.

So far it is easy to define our problem, and to see that if solvable, it must be an important problem. What the student unacquainted with modern logic will find doubtful may be the assertion that such a problem can at present be fruitfully studied. If you define this

study of the first principles of theoretical science as a branch of what is called logic, it was until recently the fashion to say that since Aristotle logic has made no progress; that that marvellous thinker had already seen nearly all of what the human mind can see regarding the structure of our thinking processes, and regarding the way in which we can use principles provisionally assumed, for the purpose of drawing conclusions from them. The principal addition that was supposed to have been made to logic since Aristotle was confined, according to this view, to a study of that inductive logic which is concerned rather with the application of our thinking processes to the discovery, the collection and the arrangement of facts, than with the structure of our thinking process itself. I wish to call attention on this occasion to the fact that this familiar assertion concerning logic and concerning its stagnation since Aristotle, in no longer true. We are today in the midst of a very vigorous and many-sided movement which interests the students of several different sciences, and which involves a rapid advance towards an answer to those very questions which I have just enumerated. We are today in a way to grow very rapidly in our comprehension of the range, of the varieties, and of the logical nature, of the fundamental conceptions upon which all theoretical science depends. We are no longer confined to the commonplace observations just cited regarding the peculiarly advantageous character of quantitative concepts and theories. We begin to know *why* the concept of quantity has the logical usefulness that it possesses. And as we come to know this, we see that the concept of quantity is one only amongst the exact and definable fundamental concepts upon which scientific theory depends. We discover that even the quantities get their logical usefulness for purposes of scientific theory from certain characters which they share with a very large number of other concepts, namely from their character of being capable of serial arrangements. and from their further character of constituting what is now called a group, with reference to certain specific operations. The series concept and the group concept thus obtain a logical place amongst fundamental concepts which permits us at once to view the quantitative theories as a special instance only amongst an infinite, but again perfectly determinate range of possible theories, some of which have

already their place in certain of the sciences, while other exact, and equally fruitful, although non-quantitative theories, are likely to become of definite use in the science of the future. We are, therefore, already on the way vastly to enlarge, but on the other hand much more precisely to define our concept of what constitutes an exact scientific theory. We are on the way towards understanding why some theoretical concepts permit of such a vast range of deduction, while others are less significant in this respect. We are becoming able to face as never before the logical question as to what we mean when we define facts as being quantitative at all. And as our view of the forms of conceptual structure which are possible for the human mind not only enlarges, but becomes more exact, we are coming nearer to the point where we can profitably study what the conditions are upon which the formation of exact concepts depends.

III.

The researches to which I refer are well known to all students of modern logic. They have come, to a considerable extent, from the mathematical side. They have been suggested, however, not only by mathematical science, but by the logical analysis of the exact physical sciences, and to some extent by the analysis of the concepts which lie at the basis of the study of the humanities, and of the historical sciences. The interest in formal logic which received a new impetus from the researches of Boole, has added itself to these other motives. As examples of inquiry of the type that I here have in mind, one may mention the well known works of Mach, and of Pearson, on the concepts and methods of physical and of statistical science, the recent books of Ostwald and of Poincarè, the various lectures on the concepts and methods of science, which were called out by the St. Louis Congress, the varied and extensive investigations of our principal American logician, Mr. Charles Peirce, the great literature which has now grown up about the theory of assemblages wihch Cantor initiated, the investigations of Dedekind upon the concepts of arithmetic, the lectures and essays of Helmholtz regarding the concepts of the exact natural science, the extensive inquiries into principles of geometry, the modern effort to formulate the concepts and purposes of historical science, the manifold controversies

concerning the office and conceptions of recent psychology, the whole range of researches in modern group theory; and in brief, all the more enlightened types of recent reflection upon the principles of science. Although myself a student of philosophy, I lay here no stress upon the contributions to this research which have in my opinion been due to the progress of modern philosophy viewed as such. There is no reason to consider the philosophers in this field as either a privileged or a dangerous class, or as for that matter easily a separable class. Cantor and Dedekind are philosophers amongst the mathematicians. I suppose it might be fair to call Mr. Bertrand Russell mathematician amongst the philosophers. I am certain that Mr. Charles Peirce is a philosopher. I am certain that Boole, although a mathematician, was guided by profoundly philosophical instincts. My interest at this moment is in laying stress upon the fact that the modern study of this subject is confined to no one branch of students, and on the other hand has so far developed that in this field one is no longer confined to the chance observations of this or of that introspective philosopher concerning what he happens to have noted regarding his personal thinking processes. The science which now deals with the morphology of theories, which seeks for their fundamental concepts, which tries to detect what unity there is amongst these concepts, which endeavors to show wherein lies the advantage which certain concepts possess for the purposes of theoretical construction, this whole science, I say, is now no longer a matter of merely private scrutiny, and of personal opinion. It is full of still unsolved problems; but it has a definite method of work. This method, like that of other sciences, is itself at once empirical and theoretical. Empirically the student of logic treats scientific theories as themselves facts which the history of science presents for his inspection. He analyzes these theories to see what their conceptual structure is. A comparative study of theories shows him the prevalence and the importance of certain types of concepts, such for instance as the concept of quantity itself. The student hereupon undertakes to analyze these various concepts into their elements, to detect what their structure is, to describe them as one would describe organisms or solar systems. He then proceeds to ask in what ways the structure of such theories is determined by the nature of human

thinking. To this end he uses means for the analysis of the thinking process which have become accessible only within the last generation. They are the means furnished by a new and now rapidly progressive science called symbolic logic. Not all the actual students of our topic have as yet made use of this instrument of research. Comparatively few are well acquainted with it. But there can be, to the initiated, no doubt of the fundamental importance of this instrument. By means of this and of other instruments of analysis, the modern student is endeavoring to trace thought to its sources, or in more exact language, to see in just what relations we place objects and ideas before us, whenever we undertake to think about such objects and ideas. The thinking process is by no means as monotonous an affair as the ordinary traditional textbooks of logic have depicted. It is worth while to add that the analysis of concepts in which the student of logic is interested is from this point of view very different indeed from a psychological analysis of thinking or from any analysis that could be carried out either by means of direct introspection or by means of the study of language. Whoever is disposed, as some psychologists are, to imagine that logic is a special branch of psychology, may well be invited to make an excursion into modern logic long enough to consider that analysis of the relations amongst the concepts: *and, or,* the concepts of *implication,* and the concept of *negation,* which the recent methods include. Such psychologists are then invited to endeavor to discover by what psychological analysis of the thinking process they could ever detect these relations.

When the analysis of the thinking process is accomplished, so far as that is yet possible, the student of modern logic is next interested in surveying the range of variation to which our theoretical concepts may be subjected. For it is a notable fact that however wide the range of liberty that we give to our thoughts, however free the range of creative activities over which we let ourselves roam, the results in the way of conceptual structure which appear to be accessible, are remarkably limited as to the number of generically distinct types which appear to be open for our consideration. Each one of these types appears, indeed, to involve, as we have already indicated, an infinitude of various exemplifications. But with all this

wealth, the definite structure, the determinate range of variation of fundamental concepts, the distinctly limited list of categories with which the logician apparently has to deal, together constitute one of the most striking results of the investigation. The thought forms, the kinds of conceptual structures which are possible, are certainly not yet thoroughly known, and their range may prove to be very far greater than we yet suspect. But the notable fact is that they appear to be built up upon a few fundamental types, which remind one by analogy of some such natural types as the vertebrate skeleton, or as the type of the insects. With endless variations in detail, each of these great types is built up in its own way, and preserves its morphological identity through its variations. The thought-types are thus not spread out in endless profusion, but apparently have a well-knit organization of their own, wherein a limited range of fundamental types spring from a common root. For instance, I have already referred to the type of structures which modern group theory defines. This type has, to be sure, an infinity of exemplifications; but all these conform to certain simple and fundamental laws. The one theory of groups consequently includes, in a sense, a very large portion of the theory of those conceptual structures which are prominent in modern mathematics. Yet there are systems whose structure is not that of the mathematical group. Their forms, again, vary in ways which we are only just beginning to understand, but which do not seem to exhibit any merely capricious variety. Unity in variety is, then, peculiarly well exhibited in the world of forms.

IV.

A few of the problems which such a survey of the morphology of the conceptual world, seems to present, may now be mentioned more in detail. That the forms of possible existence which our thought necessarily recognizes, are indeed limited in number, and depend upon as well as exhibit the necessary constitution of our thought, this philosophers long since came to feel. But the effort to enumerate such fundamental types is greatly hindered by our incapacity directly to analyze through any introspective process what the logical structures of our concepts may be. For a concept, that is a fashion of thinking, expresses a characteristic way of behavior

of the mind, a fashion of reacting to our environment. And no simple introspection can tell what such a way of behavior involves. For just as personal character cannot be discovered by looking within, but must express itself in a long and active life, before it can be fathomed, so with the forms of thought. They are methods of activity. A direct reflection does not discover their constitution and relations. These must be judged through an examination of consequences, and through a development of extensive thinking processes. For instance, if you ask a plain man how he gets the idea of number, he will reply, by counting. And he supposes that he knows by direct introspection what counting is. A psychological analysis made under experimental conditions may in many ways further dissect the mental processes which go on when we count. But how remote any such analysis is from a logical comprehension of the form of thought used in counting will become evident only after one has read such discussions as those of Dedekind in his famous essay on whole numbers, or such as Russell's and Whitehead's recent analyses of the relation between the cardinal and ordinal numbers. The relation between the number concept and the concept of quantity is again wholly inaccessible to direct introspection or to psychological experiment. Only an elaborate process of what one might call logic experimentation brings out the relation between the two concepts. The analysis of Peano, or the recent papers of my colleague, Professor Huntington, are instances of such logical experimentation. The process of experimentation in question consists of undertaking to discover what assumptions, or what various sets of assumptions, are sufficient, or are both necessary and sufficient, in order that one may be able to deduce from them the consequences which are already known to be characteristic of whatever concept one happens to be analyzing. Only by such experimentation can one dissect the thought form with which one is dealing.

It follows from our inability to detect by any direct mental inspection what ones of our concepts are the fundamental ones, it follows, I say, that the older philosophers, including Aristotle, were indeed frequently very profound and as far as they went accurate in some of their logical analyses, but could never be exhaustive, in their account of the first principles of our theoretical thinking.

Some of Aristotle's anaylses of such principles do show in fact a wonderful instinct for the essential, a logical depth of comprehension, which remains permanently marvelous as well as instructive. So, for instance, his brief but penetrating analysis of the concept of the Continuum touches upon a problem which brings him into close touch with the inquiries even of a Dedekind; and this fact about Aristotle's view of continuity has well been pointed out by Mr. Peirce.

But at the best these older analyses labored under one presupposition which was long prominent in philosophical textbooks, which was, however, long since rejected by at least one of the most famous modern philosophers, namely Hegel, and which has now become, as I think, a definitively exposed error, deeply rooted as it still is in the popular mind. This was the presupposition that the first principles of theoretical science, the fundamental concepts upon which all theoretical construction depends, are or can be known to the mind in the form of a list of self-evident principles, or of simply unavoidable and obviously necessary concepts. I say the older analyses of theoretical science mainly depend upon supposing a list of self-evident principles to be discoverable, and a list of self-evident concepts to be attainable. Even Locke, empiricist as he was, regarded the self-evident concepts and principles as indeed psychologically due to our experience, but as coming to our consciousness, after once our experience had been matured, in a shape which made them shine by their own light. But the modern logician has learned to see, that the feeling of self-evidence which frequently attends the enunciation of a principle, is commonly an indication that one has not yet learned to analyze the principle. In other words, self-evidence is a suspicious sign. It warns you that you do not yet understand the topic. If you cut a strip of paper and bring the two ends of its together to make a ring, it appears self-evident that any strip of paper must have two sides, and that in order to get from the inside of the ring to the outside of the ring by a movement which keeps your finger, or a pencil, in contact with the paper, you have either to go through the paper, or to go over the edge of the paper. All this seems self-evident or to many people may seem so, until someone shows you

the now well-known one-sided paper ring, made of an ordinary strip of paper, but so made that the two sides form but a single side. In this case the very strip of paper which has but one side, now has but one edge. And thus a universal principle which might, but for such an example, have seemed self-evident, namely the principle that a ring strip of paper must have two sides and two edges, becomes in the light of this principle, simply false; and one's geometrical ideas are hereby enlarged. So long, then, as it is self-evident to you that any ring strip of paper must have two sides, you simply do not understand the forms in question. Another case now very familiar in discussion, another case, I say, of a principle long regarded as self-evident, is the principle that the whole of a collection, must exceed in multitude any part of that collection which may be formed by leaving some of the members of the collection out. But the modern theory of infinite collections is founded upon supposing this principle to be, as it actually is, false for such collections. Thus there are as many powers of two as there are whole numbers, an assertion which follows directly from the definition of a power of two, and from the definition of whole numbers. Yet the powers of two are themselves whole numbers, and are but a portion of the whole numbers, and may be viewed as an extremely small portion in case one judges its size merely by considering what whole numbers are omitted from this collection. Upon self-evidence, then, no theory of the scope of theoretical science can be built up. I do not hesitate to say that there are no self-evident principles. And as myself, in philosophy, what is called an absolutist, that is a believer in the existence of absolute truth, I utter this assertion not in the interests of skepticism, but in the interests of truth. Single truths do not possess self-evidence, just because there are many truths which form a system, wherein each element is dependent for its nature upon its relations to the others. In general the assertion of the self-evidence of single principles has repeatedly been a foe to the progress of civilization, as it is hostile to a genuinely logical understanding of the nature of truth. The assertion of self-evidence has been used to defend almost any bulwark of tyranny from the questionings of beneficent reformers. Not upon self-evidence, therefore, nor upon a list of fundamental verities, each of which shines merely by its

own separate light, can the logical theory of science be founded. In general what we call first principles are such merely in some certain respect, or from some special point of view. Otherwise viewed, these same principles may appear as derived. And to discuss the various ways in which such derivation may be brought to light, is one of the principle problems of modern logical theory.

V.

The relative accomplishment of such a task in the case of any particular branch of logical theory involves a sort of study which the recent discussion of the logic of geometry, as well as of the logic of number theory, often exemplifies. Instead of setting forth certain self-evident axioms of geometry, or of arithmetic, the modern logical investigator undertakes to do what Russell and Couturat call defining a certain type of space, or a certain type of numbers. This process of definition, also often called the process of definition by postulates, consists substantially in saying: "I am going to describe to you the properties of a certain class of ideal entities. I do not say that these entities exist in the physical world, just as I do not deny that they exist there. But I am going to treat them simply as the entities which conform to the following definition. The definition I will state in the form of a set of principles given in order as first, second, third, and so on. I state the principles, and I define the entities in question as a set of entities such that they conform to these principles. If the principles involve no mutual contradictions, such entities are possible." Thus Dr. Veblen, in his recent essay on the so-called axioms of geometry, states twelve different principles to which certain abstract entities named points are to conform. He does not assert that these principles are self-evident. Since he is talking about purely abstract entities, which are the creatures of his definition, the principles could not be self-evident. They are true only in the sense that the entities defined are said by definition to conform to them. Dr. Veblen then shows that the laws of our ordinary geometry can be deduced from these principles as laws which hold for the defined entities. These principles, then, are sufficient as a basis for geometrical science.

A similar procedure has now become so common in discussions of this modern type, that it needs no further characterization for those who have examined any such researches. Their interest lies not in the founding of scientific theories upon any set of self-evident principles. The interest lies in showing the connection which exists amongst various concepts and principles, and in bringing to pass a logical analysis of the theory in question and of the concepts which this theory involves. No exclusive significance can be attached to any one such investigation. There are numerous, probably very numerous different sets of principles, upon which geometrical science could be founded. How far our experience of space bears out any of these principles by confirming their truth is a matter for the science of nature. Why our experience of space has these characters is ultimately a matter for philosophy. What set of geometrical principles it is convenient to use for the purposes of a textbook of geometry, is a pedagogical matter. Geometry is not deducible from self-evident axioms, since there are no self-evident axioms. Geometry is a theoretical science, since we are not confined to particular observations for our knowledge of space relations, but are acquainted with laws which enable to describe and predict our spacial experiences in general terms. The first principles of this theoretical science can be variously stated. The logical problem lies in understanding the relations that exist amongst these various statements.

Nevertheless, when a large number of theoretical sciences have been treated in this way, when their various concepts have been analyzed from various points of view, and when as is the case the forms or types of concepts which they contain have been shown to be variations of a comparatively limited number of types, such as the series type, the group type, or to speak of a more special instance, the type of the ordinary real numbers, or of the ordinary complex numbers, one is brought in the presence of a further problem which is indeed at the present time the central and characteristic problem of logical theory. It is the problem as to the unity of these forms. Fundamental ideas, in the sense of self-evident concepts and principles, do not exist in scientific

theory. On the other hand, the various inter-dependent truths and concepts of theoretical science appear to form a relatively closed system, where the special forms are infinitely numerous, but where the main types or species are comparatively few. The question in which all students of science ought to be interested, and in which students of philosophy are explicitly interested, is the question as to what common tendency of human activity it is which differentiates itself into all these forms. What a thinker does when he puts facts together, and forms a theory, depends of course upon the nature of the facts, in so far as he is trying to describe them, but it also depends upon the nature of his thought, in so far as he can only do for the purposes of thinking, what appeals to his rational interest, and what solves a thoughtful problem. A thinker, however faithful to his facts he means to be, has his needs as a thinker, and his forms of thought are his ways of satisfying his needs. He cannot merely report facts. He must interpret them. His theories are his interpretations. His world of science is his world as interpreted. It cannot be understood therefore apart from his needs as a thinker. The structure of his theories is the embodiment of these requirements of his own nature as a thinker. That quantitative science, that the principles of geometry, in whatever form they may be stated, that group theory, and that number systems, apply to his world in the regions of which he has a theoretical understanding, all this is due not merely to any outer world, which can exist wholly apart from the thinker,—not merely to such a world, I say,—but certainly also to the nature of the thinker himself. Our study of theoretical science has to be interpreted, then, as a kind of science of a thinker's ways, as an inquiry into what sort of ideal he has, as a study of the meaning of his thoughtful life, of its internal meaning, and of truth, in so far as truth is related to this internal meaning of the thinker. When we find, as we do, that the forms of thought are not endlessly variable, but are reducible to a certain range of generically different conceptual structures, we are therefore led to this question which now we face. To what are these thought forms due? What is their unity?

VI.

In an address which I was privileged to make before the St. Louis Congress I pointed out a contribution to this problem which had been suggested and in part carried out by Mr. A. B. Kempe. I have since further pursued the research which Mr. Kempe has initiated, and have published my results in a paper entitled "The Relation of the Principles of Logic to the Foundations of Geometry," printed in the *Transactions of the American Mathematical Society.* This is no place to discuss the issues involved in that paper. I want simply to indicate in a very general way one point regarding the kind of result which seems to me to be already in sight, although the matter is still very incompletely worked out. The different characteristic forms of thought to which I have referred, are distinguished by the various types of relations which these various forms exemplify. Thus the characteristic ordinal relation of descriptive geometry is the relation called "between"; and Dr. Veblen has shown how in terms of this single relation, and of the assumption of the existence of appropriate objects or entities, one could state all the principles that are needed as the foundation for geometry. The characteristic relation of the world of quantity, the relation of "greater and less," is a relation which in combination with the triadic relation that is involved in the ordinary operation of addition, is sufficient to give form to the principles of algebraic analysis. In brief, then, each theoretical science has its own characteristic set of relationships. When so viewed these relations stand by themselves, as if they were separate facts in the natural history of the forms of thought. Relations may be classified, just as truly as birds, or as bacteria may be classified. There are relations dyadic, triadic, n-adic, there are relations symmetrical, unsymmetrical transitive, intransitive. These varieties of form in the world of relation, when thus viewed, seem ultimate and irreducible. Yet I do not think that anybody finds it self-evident, or axiomatic, that only these relations should be possible. I do not think that we have any warrant for saying on the other hand that the sorts of relations which exist are capable of a simply limitless and a capricious variety. The concept of a relation is to my mind, as to the minds of a good many of my colleagues, something that is intelligible only in terms

of the activities of our own thought. We understand relations, because of our own thinking processes we can at once depict, and in a sense reconstruct or create them. The types of our own construction, of our own thoughtful activity, are therefore the relational types. If we are to understand, then, the unity and the system of relational types, we must see how their varieties are related to our own activities as thinkers.

Now, however, relations are known to us not only as existing in teh world of numbers and of geometry, but as present in the purely logical world, the world of classes and propositions, of syllogisms, and of reasonings in general. I have already mentioned what some of the logical relations are. They are relations such as are expressed by the words "*and*," "*or*," "*not*," "*implies*," and so on. These relations are as fundamental and as simple as are our thinking processes themselves. We learn about them not through our senses, but through our activity as thinkers. Now what Kempe's research suggests, and what my own line of research has tended I hope to bring a very small step further on the road towards definition, and confirmation, is the thought that such geometrical relations as "between," such relations as "greater" and "less," and even such relations as are fundamental in group theory, are capable of being interpreted as instances, as consequences, or as partial views, of the fundamental logical relations themselves. Kempe has shown how a logical class can be viewed as "between" two other classes and how the geometrical "between" can be regarded as a special instance of this logical "between" I have shown how the system of Dr. Veblen's principles of geometry could be brought into definite connection with the relations which characterize a system of logical classes. The whole research in question is still in a very elementary stage, but enough has been done, I think, to make it at least probable that whoever comprehends the most fundamental logical relations, such as a child begins to comprehend when it first says "no," that is, whoever comprehends such relations, as "and" and "or" and "not," and the relation of implication, has already in his hand the means for developing the fundamental concepts of all of the exact sciences, since the relations of these exact sciences are more or less

complex variations and recombinations of the fundamental logical relations themselves.

Meanwhile, however, the fundamental logical relations are characteristic not only of our world of thought, but also of our world of action. For will-acts involve acceptance and refusal, affirmation and negation, a consciousness of consequences, a facing of alternatives, a union of various acts in one act; so that the logic of action is in form precisely the same as the logic of abstract thought. In brief, so far as I can see, the trend of the modern study of the principles of theoretical science is at present towards proving that all the forms of conception used in exact science are but expressions of the characteristic types of will activity of which we as voluntary agents are capable. We thus conceive the structure of the world in terms of the structure of our own types of voluntary activity. The forms of our will determine the types of our theoretical concepts. We define facts, so far as we theoretically comprehend them, in terms of the nature of our wills. The view of the logical source, and of the internal structure of our concepts which is thus suggested, is closely akin to what is nowadays called pragmatism. But to my mind any pragmatism rationally thought out becomes philosophically speaking an absolutism. Yet with that philosophical question we have here nothing to do. The result of our modern study of logic is certainly to give us no less respect for facts, than we get from the study of nature. But the facts with which the logican has to deal is the fact that as a man willeth, not only so is he, but such are his theoretical conceptions. The whole trend of his theoretical science consists in his effort to find in the universe, in the end, the expression of his own will. His fancies, his capricious will, his temporary hopes and hypotheses, he learns to resign; and he calls this resignation a submission to external facts. But this submission itself is an action of the will, a rational act, but also his own act. As he proceeds in the work of his thinking, he is, as Kant long ago said, endless interpreting the world in terms of his own thought. But the forms of his thought, these prove to be ultimately the forms of his voluntary activity. Our modern unification of the concepts of theoretical science looks then towards viewing all the fundamental types of relations as identical with the types of the purely logical relations,

such as come to mind when we assert, deny, infer, or otherwise deal with the relations expressed by the words "and," "or," "not," "implies" and a few similar terms. But these forms of relations are themselves the forms in which our will embodies itself. So that our theories of the universe tend to be like the other works of our civilization, the result of a long struggle with nature, by means of which, when we win at all, we attain the end of finding our own will expressed in the order of the controllable facts. Some such consideration the modern study of the principles of theoretical science seems to me to enforce; and from this point of view I regard this study as belonging to what Franklin had in mind when he used the term "useful knowledge."

CAMBRIDGE, MASS.,
April 18, 1906.